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**THE**  
**GLASGOW MEDICAL JOURNAL.**



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EDITED BY

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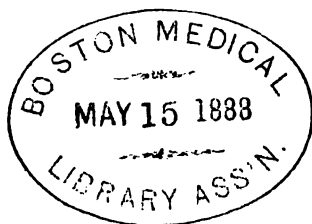
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## ORIGINAL ARTICLES.

### ON HÆMORRHAGES FROM THE UNIMPREGNATED UTERUS.

By JAMES STIRTON, M.D.

*(Being his Introductory Address as President of the Obstetrical Section of the Glasgow Medico-Chirurgical Society.)*

ALTHOUGH not unmindful of the old proverb, "Qui s'excuse s'accuse," I must crave your indulgence on this occasion. What with professional work, which has been rather worrying for the last two months—what with having to implement my engagements in other departments of science, I have not had time even to turn my thoughts to gynæcological subjects, much less to elaborate a paper for so learned a Society as this, and it was only when our Secretary, after listening to my bemoanings, slowly, but sullenly, stiffened still further his otherwise rather stiff back-bone, and refused to interfere with the existing arrangements as to the succession of the subjects of debate, that I began to awaken to the awkward position I was in. Well, then, a hurried retrospect of the field of observation finally determined me to remodel and rearrange an old subject of debate—viz., the hæmorrhages external as well as internal in connection with the unimpregnated uterus. The latter are usually subdivided in the more modern works into menstruation, menorrhagia, and metrorrhagia; the former into endoperitoneal and exoperitoneal (I altogether object to *intra-peritoneal and sub-peritoneal*).

No. 1.

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Vol. XXVIII.



In the first place, permit me by way of preliminary to make a few remarks on some uterine peculiarities, or at least what in my opinion are uterine peculiarities. 1st. I believe the uterus and Fallopian tubes, including their fimbriated extremities, are *erectile* under excitement—the uterus under sexual excitement as well as menstrual, and the Fallopian tubes under ovarian excitement or during ovulation.

The circulation in the uterus is such as to favour this view. 1st. The presence of sinuses (which can be demonstrated even in the unimpregnated condition) into which the blood can be poured until they are fully distended, affords an analogy with the circulation in other erectile organs. 2nd. This excitement is occasionally rendered evident by the speculum, when the cervix is seen to take a more rounded form as well as to swell, and the os externum to open somewhat and, although very rarely, to eject the clear Nabothian mucus. Rapid relief from all such excitements is afforded from the peculiar anastomosis of the large veins with thin walls on the outside of the uterus, more especially those surrounding the cervix and called the uterine or cervical plexus. Corresponding to this, there is a plexus in connection with the ovaries called the ovarian or pampiniform plexus, and it is remarkable that the two sets are curiously connected by intermediate branches. More of this anon.

Under normal, and accordingly healthy, conditions, this disposition of the uterine circulation permits rapid engorgement, and affords means for as rapid depletion; and, if the two processes are mutually adapted, or balanced, healthy action is maintained. I may state that I have not satisfied myself of the existence of valves in either of these plexuses or their continuations, and the presumption is, that obstruction to the return flow of blood tends to engorgement and consequent dilatation of these veins, with thinning of their walls and increased liability to burst.

In connection with this, permit me to suggest that the reason why the left ovary is nearly twice as often congested, &c., as the right, may, in part, be due to the return flow of blood from it not being so free, or rather more liable to be clogged, inasmuch as the left ovarian vein is emptied into the left renal.

First, as regards *menstruation*. What is it? It is usually defined as the external manifestation and proof that a cycle of changes in the generative system has occurred and been nearly completed, and that it is usually coincident with the maturation and at times with the extrusion of an ovum. But

why should this bloody manifestation form so necessary, or at least, so constant a concomitant of the series of phenomena?

In the vast majority of the lower mammalia there is no such flow, but the clear Nabothian mucus is manifest externally in many. In other words, the bloody flux is not necessary in them towards the relief of the body of the uterus. I have often pondered over this peculiarity of menstruation in the human subject. That civilisation has indirectly to do with it is, I think, beyond doubt, more especially when account is taken of indiscriminate intercourse. Besides, analogy in the case of different races goes far in affording another clue to this peculiarity.

It is well known that a few of the lower animals have this bloody menstruation—viz., such as are domesticated, as rabbits kept in a warren for several generations, and fed on rich food; also the sow and elephant, probably for similar reasons. What is, however, somewhat peculiar, and tends to strengthen our theory, some tribes of monkeys in India and elsewhere menstruate pretty frequently during the warmer months. Such monkeys are gregarious, and show besides a wonderful amount of organised sociality, and indicate a high advance in monkey civilisation.

Again, in the colder countries of Europe, such as Northern Sweden, whose inhabitants lead quiet, unexciting, uneventful lives, yet happy in their simplicity and frugality, menstruation occurs late, as at 17 or 18, and, not unfrequently, not until after marriage, or it may be not until many months after the birth of the first child. In such instances, the delay or absence of the flow is not attended by such deleterious effects upon the system, as is the case in women brought up in warmer climates and exposed to the exciting life of modern civilisation. In such women the flow is, as a rule, much more scanty, and, what is significant, much more tenacious; in other words, more mixed with the cervical mucus so characteristic of the heat or rut of the lower animals. Again, in South Africa, at a latitude where the winters involve comparatively cold weather, there exists one of the aboriginal races (not the Zulus, who are a comparatively modern intrusion from the more northern or warmer regions) who inhabit pits or excavations beneath the surface of the ground, and whose habits and intercourse generally differ little from those of the lower animals. The females of this race have only irregular periods of menstruation, and, what is remarkable, give out at the first blush of womanhood only abundant mucous secretion as in the lower animals. It is only when indiscrim-

inate intercourse has taken place for some time that blood appears, and then merely in comparatively small quantities. My limits will not permit me to give more extended instances in the same direction, but sufficient has been advanced to afford a presumption, at least, of the truth of my former statement, to the effect that civilisation has much to do in determining such a local flow of blood as to necessitate a monthly depletion in the way of menstruation.

I have spoken elsewhere of the evils to the system attendant upon an arrestment of the menstrual flow; permit me to-night to indicate those resulting from irregularities as to quality and quantity of the blood; and, in the first place, this flow cannot be said to be the direct effect of ovulation, inasmuch as it may be induced by other causes, *e.g.*, strong emotions, more especially those of joy—sexual excitement in the newly married, although the opposite condition of arrestment is more frequently true—a day or two after operations in the abdomen, as after removal of one ovary, &c., &c. On the contrary, the flow is liable to be arrested by nearly as many and varied causes. In other words, gentlemen, the nice equilibrium of forces so necessary to the due sequence of the phenomena of menstruation is very easily disturbed, and may in truth be said to be in the language of physics unstable.

Such disturbances as those enumerated above are due to the nervous system. The more important disorders of menstruation are on the other hand due to disturbances of the circulation. Permit me to enlarge somewhat on this point. You have to deal with an organ whose circulation is capable of being quickly reduced to a minimum under the action of emotions, or as quickly engorged with blood under corresponding influences. Nay, the same emotions may induce opposite conditions of the circulation, and this often within 24 hours. One can scarcely be long in practice without experiencing the truth of these assertions; excessive joy arresting the menstrual flow for 12 or 24 hours, to be succeeded by a corresponding increase at the end of the period or *vice versa*.

In fact, gentlemen, the more you study the uterus as regards its hæmorrhages, the more are you convinced that it behaves exactly like an erectile organ. Now, so long as the influences that act upon the uterus are emotional, or speaking generally, through the nervous system, this disturbance of its circulation is in general not attended with any but temporary evil results. But along with these emotional influences, or apart from them, introduce other elements of disturbance of the circulation, and you have pathological hæmorrhages.

These other elements of disturbance may obviously be divided into two grand classes—viz., those of disturbance to the egress of blood from the uterus, and those conducive to obstruction to the venous return. Let us speak of these in succession.

As my purpose to-night is not to give a comprehensive view of the known causes, I shall only mention some of the more common conditions of the mucous membrane attended with a proclivity to hæmorrhage:—

1. Subinvolution, inasmuch as the interstitial deposits prevent the due contraction of the organ, and the sinuses are not closed. The hæmorrhage in such a condition is generally intermittent, or rather remittent.
2. Granular degeneration.
3. Scorbutus.
4. Arthritic diathesis.
5. General plethora.

In connection with this last condition of system I beg to state that where blood is rapidly formed, as in the *female sex generally*, the uterus affords an adequate safety valve for the surplus. Accordingly, in persons of plethoric habit such flows should never be checked, otherwise enormous obesity is the result. What might appear under ordinary circumstances as menorrhagia is nothing more than a due relief to the system. The flow may be troublesome, but it is necessary.

The second class of disturbing elements I mean to treat of at greater length—and, indeed, this class is the main item of consideration; but at this stage permit me to reiterate the relative positions of the parts of our subject. We have an organ subjected to very varying degrees of vascularity—a vascularity which in these days of advanced civilisation can only be relieved monthly, at its maximum degree, by a depletion of a peculiar kind from the mucous membrane. An organ, besides, anatomically isolated from the rest of the economy; and, moreover, having its vascular supply but meagrely continuous with that of the neighbouring organs. As a consequence, this vascularity is subjected to a wide range of tension—a tension which must tell severely on the venous return flow as well as on the capillaries, and must tend to expansion and dilatation, more especially in the branches of the two venous plexuses around the cervix, and below the ovaries. These veins (particularly those of the latter plexus) are unsupported on either aspect throughout their course, are nearly throughout valveless, and have thin walls as I have already stated. In an upright posture, as in man, this tension is very considerable, inasmuch as the column of return blood

is perpendicular or nearly so; whereas, in the lower animals the course of the venous return is nearly horizontal, and the tension accordingly much less. This tension, it must be remembered, is greatest at the lowest points—viz., at the two venous plexuses. If, in addition to all these adverse conditions, there are difficulties to the venous return from disease of the *liver*, *heart*, or *lungs*, you can readily perceive that this tension on the plexuses is increased manifold. Under these circumstances it can scarcely be matter of much surprise that rupture of one or other of these plexuses should occasionally take place. Suppose, for instance (an instance which I have seen), that a woman about the period of maximum turgescence of the uterine vessels is called upon, in the way of her household duties, to lift a heavy weight or drag out a heavy trunk from beneath a bed, and, moreover, that the ovarian plexus is in a varicose state, and that perhaps she has stenosis of the mitral valve, or it may be of the tricuspid, is it matter of much surprise that a rupture of the veins alluded to should take place with or without rupture of their peritoneal lining, the broad ligaments? For my part, I wonder such a horrible accident does not take place more frequently than it does.

Suppose, on the other hand, that the obstruction is to the egress of blood from the uterine mucous membrane, as in acute endometritis from cold, or rather that a sudden arrestment of the menstrual flow is produced by such acute endometritis, more especially in one of the arthritic diathesis, you may readily conclude that the sudden revulsion of blood upon the uterine venous plexus must be severe, and, in certain conditions so severe as to cause rupture and hæmorrhage to an enormous extent. In this instance, also, the peritoneum may or may not rupture. Under this last condition of system—viz., the rheumatic, I may say that the peritoneum generally remains intact.

I have chosen to put a series of hypotheses, rather than to state dogmatically any theory of these uterine hæmorrhages, for the simple reason that my case book contains records of only three, or at most four, where corroborative evidence has been obtained in the direction indicated above.

My object is mainly to call attention to the probable sources of the blood supply. Hitherto, in books a multitude of possible sources have been huddled together indiscriminately, and different writers have taken up one set of causes to the almost entire exclusion of others. Considerable friction has accordingly been and is still engendered in medical coteries, more

especially as portrayed in our periodicals. Of course, there are several very evident sources, as rupture of the uterus, rupture of the Fallopian tubes in tubal pregnancy in so-called apoplexy, and rupture of the ovaries, &c. In all such the blood is endo-peritoneal. As, however, such complications are excessively rare (and happily so), we may dismiss their consideration in the present discussion, more especially as there is no element of doubt concerning them.

The main contention is whether the blood is exo-peritoneal or endo-peritoneal in its first instance, and all sorts of plausible theories have been advanced in support of both positions. Some have scouted the notion that the peritoneal lining as such can bear tension to any appreciable extent, and they detail experiments on the dead body in support of their pretensions. Others, again, have shown clearly that the *living* peritoneum is capable of considerable tension and even elasticity. This I have been enabled to corroborate. Not later than a week ago I evacuated, by means of the aspirator, through a fine canula, a uterine hæmatocele formed during labour—how formed I could not determine. About a week after delivery it formed what felt like a wen or bump the size of the closed fist on the outside of an otherwise regularly outlined, well contracted uterus. As peritonitic symptoms threatened to supervene it was decided to aspirate. The case is likely to recover. In this case there was no room for doubt as regards the exo-peritoneal situation of the blood. To suppose otherwise would be to contravene the law of gravity, inasmuch as the bump was situated near the fundus uteri. Leaving this rather unprofitable part of our subject, let us inquire next what are the most frequent sites for the extravasation of the blood?

In my experience much the most frequent is the situation of the cervical plexus. Where pressure on the veins is considerable from obstruction, more especially to the venous return, the peritoneum is very often burst through, and a vast amount of blood is poured out, with death, as a rule, within 48 hours. In other cases, the peritoneum withstands the first gush, and even aids materially in arresting the flow as well as acting as a preventive to another burst. Such cases terminate well. They are easily diagnosed by the finger in the posterior *cul-de-sac* of the vagina, where a dense, somewhat fluctuating, mass is felt at first, which, however, soon becomes nearly solid, and conveys the impression of a hard, non-yielding wall. A certain proportion of these cases have repeated outbursts of hæmorrhage, with the usual concomitant

general symptoms attendant upon sudden loss of blood, as well as the common local symptoms. Each successive outburst forms a mass distinct from the others, and may ultimately be felt through the abdominal walls as a separate and more or less well defined mass. I have counted no less than four such masses in the abdomen, affording the presumption that at least five extravasations had taken place. Now, on the supposition of the blood being poured out into the general cavity of the peritoneum, there could not be any such separate nodulosities felt, and *felt, remember,* prominently. In one instance the patient herself told me she felt the bump rising upwards quite distinctly, as she had been accustomed to put her hand on the lumps (as she termed them) in order to ascertain whether they were lessening. In fact, she kept her hand on the spot until she fainted or nearly so.

Another determination of the blood in this class of hæmorrhage is downwards in place of upwards or laterally; downwards beneath the peritoneum until it reaches the lower part of the posterior wall of the vagina, through which it bursts, as a rule, and is evacuated per vaginam. This class is by far the most favourable, and, curiously enough, there is seldom more than one extravasation. I have seen at least seven of these cases, and all did well. In one a second extravasation occurred on the street while the patient was stooping to pick up a parcel. Had I not known the origin of the previous attack I could scarcely have diagnosed such a cause, &c.

In extravasation from the ovarian plexus the relative position of parts will easily enable one to trace the probable direction of the hæmorrhage. In only one instance could I satisfy myself of this as a probable or likely source of hæmorrhage. There were two hæmorrhages. The first was manifested in the left broad ligaments, and the mass could at the same time be felt at the left apex of the vagina. After the second extravasation the uterus, broad ligaments, &c., were fixed in one immovable mass, which nearly filled the pelvis. In this case there was no rupture of the broad ligaments, although their distension must have been enormous. She recovered without any affection of the bowels in the way of obstruction, &c., a fact which, in my opinion, goes far towards proving the exo-peritoneal nature of the extravasation.

## LECTURES ON THE DISEASES CLASSIFIED AS TABES MESENTERICA.

By W. T. GAIRDNER, M.D., LL.D.,  
Professor of Medicine in the University of Glasgow.

(Continued from page 408, vol. xxvii.)

### LECTURE IV.—PRACTICAL CONSIDERATIONS; PREVENTION AND CURE.

I WILL now attempt to trace out for you such a narrative of the origin and progress of these disorders of the abdomen, whether in children or in adults, as may guide you with respect to their prognosis, and also their management in detail. Two distinct classes of cases may be adverted to:—

In the *first*, notably illustrated by the case of L. C. (see Appendix) the abdominal swelling occurs as either absolutely or very nearly the first symptom of ill health; it may be without appreciable pain or uneasiness; or, at least, with pain so slight as to be practically overlooked. There may be no diarrhœa at any time; there may even be constipation. The tongue may be clean; the appetite fairly good. It is, however, not usual for the disease to last long under such circumstances, without a notable change in these respects, and without an appreciable degree of emaciation. The temperatures may present every possible degree of variety; they may even be entirely undisturbed throughout. More usually, however, the evening temperatures on the whole, are relatively elevated as compared with the morning, be it ever so slightly. In some cases, there is a pretty regular diurnal oscillation of one or two degrees or more, the evening temperatures being to this extent in excess of the morning, which may or may not slightly exceed the normal. In such cases, and when the strength and bodily condition are well maintained, the prognosis may be pronounced relatively good, even when the physical lesions in the abdomen are quite well defined. And if under these circumstances evidences of moderate fluid effusion occur, followed by increased resistance and dull percussion over the middle abdomen, still there is no reason to despair of effecting at all events a practical, if not an absolute, cure. It is quite unusual for the amount of fluid to become such as to raise any question of paracentesis. Indeed I can only remember, in eleven years' experience in the Western Infirmary, two or three cases where the question of



surgical interference presented itself in a practical shape. In two of these, at least, the operation appeared to be urgently demanded; but the results were not encouraging; and, although I am far from believing that the condition of these patients was made worse by paracentesis, yet, on the whole, the effect upon my mind was to produce an impression that, unless absolutely demanded by urgency, it is better to abstain from the use of the trocar. In the majority of cases the fluid effusion spontaneously subsides within a reasonable period, and nothing is gained by the attempt to hurry the process.

In the *second* class of cases the abdominal distension has either been preceded or is accompanied by other disorders, the significance and the gravity of which may vary almost indefinitely. In some cases there is pretty severe pain, with tenderness on pressure, such, in fact, as to suggest a sub-acute, if not an acute, attack of peritonitis. In others there is diarrhoea more or less long continued. In some, but by no means in a large proportion, there is vomiting; in the majority of cases, however, vomiting is either not present, or is altogether occasional and accidental. The physical alterations in this group of cases, to percussion and palpation, may differ but little from the former; the prognosis, however, is distinctly less favourable. And, in not a few of these less favourable instances, the temperatures are persistently elevated; diarrhoea is more or less constant, and accompanied by pain; the tongue becomes coated, or perhaps still oftener red, with markedly injected papillæ; and the emaciation is considerable from the first. The essential difference between the two groups as here briefly indicated is, that in the former there may be no disease of the mucous membrane, while in the latter ulceration, more or less considerable, of the glands of Peyer, or of the solitary glands of the ileum and colon, has preceded or accompanied the abdominal swelling. The presence or absence of mesenteric tubercles makes very little difference as regards the symptoms. In the majority of the latter group, and in almost all the fatal cases, they are present; but so inextricably involved in the other lesions, whether of the mucous or serous membrane, or both, as to lead to the probable inference that they are of quite secondary importance. At all events, there is no reasonable ground for adopting the rule of many of the French authorities, of confining the use of the name *carreau* to the cases of mesenteric glandular tubercle. It is not improbable, however, that a distinction insisted on by M. Guersent, and, on the other hand, disallowed

by Rilliet and Barthéz, may be considered as corresponding generally, though by no means in every detail, with the symptomatic differences of these two groups. "Mesenteric tubercles," writes M. Guersent,\* "present themselves under two very distinct aspects, which must necessarily have a very different influence on the abdominal organs, and consequently on the vital phenomena thence arising. The tubercles may be without any kind of inflammation of the surrounding parts, or they may be accompanied by a true phlegmasia of the glands, and sometimes even of the intestinal mucous membrane in the neighbourhood of the diseased glands. In the first case, they (*i. e.*, the tubercles) are indolent; in the second, they are ordinarily painful." And he proceeds to show, accordingly, that between the *carreau indolent* and the *carreau inflammatoire* there is all the difference between a disease with hardly any distinctive symptoms and one having the ordinary and, to a certain extent, well known symptomatic characters of the *tabes mesenterica*.

The error here is, if I mistake not, what I have already pointed out in a preceding lecture—*i. e.*, that M. Guersent, while admitting in detail that mesenteric tubercles have no distinctive symptoms at all, still adheres, most illogically, to the old tradition which identifies them exclusively with the *carreau*, and considers all other lesions as purely accidental. MM. Rilliet and Barthéz, following in the same track of tradition, but describing, with the greatest care, the anatomical condition of the diseased mesenteric glands, maintain (no doubt quite correctly from this point of view) that it "seems impossible to find, in the pathological anatomy, a point of departure for the division of the *carreau* into two species—painful and indolent. Pathological anatomy," they remark farther, "does not justify this manner of looking at the subject, and symptomatology is still more opposed to it."† But by separating entirely, in two distinct chapters (xv and xvii), the tuberculisation of the peritoneum (*phthisie péritonéale*) and the tuberculisation of the mesenteric glands (*carreau*), MM. Rilliet and Barthéz have, in effect, thrown away the opportunity of presenting a complete nosological picture of disorders which are so constantly seen in combination that it is practically impossible to dis sever them. The real basis of the distinction of the *carreau* into two varieties is to be found, if at all, in the difference above mentioned.

\* *Loc. cit.*, pp. 316, 317.

† Vol. iii (2nd edition), page 810.

The clinical distinction between the two classes or groups of cases above described (which, however, must be considered as passing into each other by all sorts of intermediate gradation) is sufficiently manifest. In the former, the prognosis is relatively good; but, on the other hand, it is to this group that the remarks chiefly apply which I formerly made to you as regards the difficulty of defining their strict pathological position, and especially in determining how far they belong, or not, to the tubercular class. All that can be affirmed with certainty in the meantime is that such cases occasionally, and by no means infrequently, get apparently well. And, moreover, their antecedents, hereditary and other, are often such as to justify the hope that at least in some of them the cure may be permanent. On the other hand, much of what I have to say as regards the preventive, hygienic, dietetic management of such cases, applies less to this group than to the other and more formidable one; because a disease emerging, as it were, directly out of a state of apparently good health scarcely admits of the application of such anticipatory treatment. Such cases may perhaps belong to the variety described by Bauer \* as *chronic diffuse peritonitis*, in its variety of *latent general peritonitis* resembling ascites; but, if so, the information before us as yet gives no insight into its causes, such as would suggest any practical distinction of it from the corresponding class of tubercular cases. "The symptoms which are present in the beginning of the disease (according to Bauer), are, as a rule, very trivial, anything the patient complains of being general and undefined. Dull pains occur in the abdomen, which are increased on pressure and on bending of the body. The patient's general health is out of order; he feels tired, and his appetite is bad. Going up stairs, especially, is frequently the source of an unpleasant sensation in the abdomen. Months may intervene between these primary, scarcely heeded, symptoms and the complete development of the disease; in other cases this stage, which Galvagni describes as prodromal, is shorter." . . . Even in the stage of effusion, "the phenomena are in no way very urgent, and are such as might, for the most part, be quite as fully produced in simple mechanical ascites. . . . There is something remarkable in the insignificance of the pain in many, and the complete absence of it in some few cases of chronic peritonitis. The temperature is often found to be quite normal; in other cases there occur evening exacerbations

\* *Loc. cit.*, pp. 316, 317, *et seq.*

of a moderate degree. The pulse, on the other hand, is almost always quickened.\* It is needless to point out the close resemblance between Bauer's description of what he regards as a very rare disease, and that with which we are at present concerned. Upon the whole, without excluding from view in further remarks the first group of cases, it is the second that we must keep chiefly in view in all that concerns treatment, and especially prophylaxis; it being understood, however, that the principles of management applicable to the second group are also to be kept in view in the first as far as may be.

If an infant, or a young child at any age, appears to be out of health, or emaciating, or disturbed in any way as regards the abdomen, it will always be proper to suspect, at least, the beginnings of this disorder. MM. Rilliet and Barthez, like almost all the other writers on the diseases of children, insist on the differential diagnosis between simple enlargement of the abdomen and that of tubercular peritonitis. And Guersent, as we have seen, has also described the enlarged abdomen of debilitated or rickety infants as being quite distinct from that of mesenteric disease, or *carreau*. The distinction may be admitted as a pathological fact, and yet some of the grounds of the diagnosis cannot be accepted without demur. "The consideration of the age alone," say MM. Rilliet and Barthez, "would suffice to prevent mistake: peritoneal phthisis being extremely rare among young infants, while, on the contrary, rickety distension is frequent at this period of life" (page 791). A similar remark is made by Guersent as regards mesenteric disease. But these statements are scarcely borne out by the facts; for in the article on causes (p. 794), MM. Rilliet and Barthez adduce no fewer than 11 fatal cases of tubercular peritonitis as occurring between 1 and  $2\frac{1}{2}$  years, as against 26 between 3 and  $5\frac{1}{2}$  years; 40 between 6 and  $10\frac{1}{2}$  years; and 9 between 11 and 15 years; thus showing that the conditions which dominate tubercular peritonitis, if not the actual fully developed disease, must, from the practical point of view, be reckoned with as in operation from the very earliest ages. For it is hardly necessary to point out that the 11 infants who died within the first three years of life, and the  $11 + 26 = 37$  in whom the disease reached its fatal term within the first six years, were in all probability subject to the causes of it from a very early age; if not from birth, at least from the commencement of dentition, and therefore quite within the period when no practical distinction can be

\* Ziemssen's *Cyclopædia* (American Translation), vol. viii, p. 299.

established on this ground between the rickety and tubercular forms of enlargement. Moreover, in seeking to lay down prophylactic rules it would be impossible, or at least would be highly inexpedient, to limit these to the developed disease. Statistics founded on fatal cases are therefore only applicable when we give due consideration to the fact that these fatal issues are the sequel and last extremity of a more or less protracted course of impaired health, the first stages of which it is very desirable, if possible, to intercept. The same remarks apply to the statistics by Rilliet and Barthez of mesenteric tubercles (*loc. cit.*, p. 820). These were found in 27 cases at from 1 to 2½ years, as against 41 cases from 3 to 5½ years; 55 cases from 6 to 10½ year; and 21 cases from 11 to 15 years. Here again, therefore, although the greatest predominance of fatal results is between 6 and 10½ years, the numbers given at earlier ages necessarily carry us back to the very earliest infancy, if we are to entertain at all the idea of prophylaxis. And it is further to be remarked that, at all the ages given, but much more at the earlier ages, the numerical preponderance of the male sex is very notable, amounting to about double in the very earliest ages, and from this to about a third greater in numbers as childhood advances. I have not found any corresponding statistics of the rickety distension of the abdomen, for the obvious reason that this is rarely fatal; but it is generally admitted that rickets, while occasionally congenital, and not unfrequently developed during the first six or twelve months of life, attains its greatest frequency, and possibly its highest active development, during the second year, or, speaking broadly, during the progress of the first dentition, while the authorities differ widely as to its relative frequency in the two sexes.\* It is, therefore, obviously impossible to found on mere considerations of age and sex in establishing such a differential diagnosis as shall be of practical utility in the early stages of either disease. This point I regard as one of great importance; because it has been usual to assume (in my opinion without any sufficient foundation) that all enlargements of the abdomen, and most of the catarrhal affections of the mucous membrane occurring before or during the first dentition, are to be taken as non-tubercular; and even that there is a kind of antagonism, or mutually exclusive element of difference, as between the rickety and the tubercular constitution. Thus, M. Guersent declares that rickety infants are very rarely the subjects of mesenteric disease or *carreau*. The statistics of MM. Rilliet

\* Ziemssen's *Cyclopædia* (Senator), vol. xvi, pp. 172, 173.

and Barthez are entirely opposed to this assertion. I am strongly persuaded, on the contrary, that *rickety infants who die* would, like other infants, be found in a large proportion tuberculous. It is sufficiently obvious that those who recover, or who present at more advanced ages the permanent rickety deformities, are precisely those who may be said to have escaped death *because* they were non-tuberculous, or, at all events, not tuberculous in a high degree.

Whenever, accordingly, in a very young child there is a manifest disturbance of health affecting the general nutrition, especially if this is associated with diarrhœal disease, or with colicky pains in the abdomen, and habitual feverishness, if not explained by any accidental cause, or by any endemic or epidemic condition generally prevailing, it is, at least, safe as well as expedient to adopt measures founded on the presumption that such manifestations may be the harbingers of organic changes which may issue in *tabes mesenterica*, or in some of its congeners. In the well known essay of Dr. Cheyne on the Atrophia Ablactatorum, or "Weaning Brash,"\* there is not, indeed, a well ordered account of the pathological anatomy, nor is there any clear recognition of tubercular disease concurring with the lesions described; which, indeed, the author's theory leads him to associate more with hepatic derangement than with the changes in the mucous membrane of the intestines. But in one at least of his dissections, Dr. Cheyne has especially figured "the glands in the root of the mesentery much enlarged" (page 49). This child was under 13 months old; was weaned at 11 months, and about a fortnight after weaning, diarrhœa came on. The fatal issue was due to convulsions; the purging having ceased, owing to the administration of remedies. And the author remarks that "the original disease had by no means arrived at so great a height as I have seen it. The emaciation was not so great as usual, nor the purging nor derangement in the alimentary canal so determined" (p. 47). It is quite obvious that the disease in this case was rather acute, and in all probability, was not definitely tubercular. This may, indeed, be the usual fact in cases of this kind; and yet it is surely reasonable, in view of the facts stated above, to argue that when, from hereditary causes or otherwise, young children are predisposed to tuberculous forms of disease, the first foundations of such diseases may often have been laid at

\* *Essays on the Diseases of Children, with Cases and Dissections. Essay II.* By John Cheyne, M.D. Edinburgh, 1802.

the time and in the manner here set forth.\* In like manner it is difficult to believe that the various forms of diarrhœal disease in young infants, especially those that are characterised as *inflammatory diarrhœa* or *entero-colitis*, are not more or less closely associated as causes, if not as coincidences, with the derangements of health that, at somewhat more advanced periods of childhood, bring about deaths that are frankly tubercular. It is not necessary, in order to maintain this position, that we should assume the existence of tubercle as a demonstrable fact in many of the fatal instances of these early acute forms of diarrhœal disease. No doubt, innumerable cases of simple diarrhœa, of epidemic diarrhœa, or of cholera infantum, and of entero-colitis, attended by destructive lesions, chiefly of the large intestine, may occur altogether apart from tubercle; and yet, as we have seen, abdominal tubercle is very far from being unknown in early infancy. Scarcely any one doubts the influence of certain acute fevers in arousing the latent proclivities to tubercular and scrofulous diseases of the chest—*e.g.*, measles, whooping-cough, and epidemic influenza. Why, then, should it be doubtful that the acute and sub-acute disorders of the intestinal canal which occur during, or even previous to, the first dentition, may reasonably be supposed to have a like influence as regards abdominal tuberculosis? In the valuable chapter on entero-colitis in Meigs & Pepper's *Practical Treatise on the Diseases of Children*, many interesting details will be found as to the lesions in question, and their association with climatic and epidemic conditions. It will be sufficient for my purpose to refer you to this easily accessible work.

It follows, then, that in all cases in which, from hereditary antecedents, or from delicacy of constitution acquired soon after birth, a child may be reasonably suspected of a tubercular predisposition, the utmost care should be taken that, if possible, during the specially perilous season of

\* "The mesenteric glands are enlarged," writes Dr. Cheyne, in giving the results of his general experience in fatal cases of the "weaning brash;" "nay, in some instances, inflamed. May not this proceed from the acrid nature of the alimentary matter to which their absorbing mouths are exposed? May not the *tabes mesenterica* often arise in this way?" The remarks which follow, at p. 28 (note), show conclusively that, without having definitely adopted a tubercular pathology of these disorders, and having, moreover, peculiar and individual views as to the relation of the biliary secretion to the *atrophia ab lactatorum*, Dr. Cheyne was, nevertheless, fully impressed with the probable pathological continuity of the more acute disorders of early infancy described in this essay, with the more chronic and scrofulous ones of childhood generally. Case III, intended to demonstrate this, is too long to quote here, but may be referred to at pp. 28-30.

weaning, and during the whole progress of the first dentition, nothing should be done to bring into activity the latent defects of constitutional habit which might afterwards assume the form of some of the diseases with which we are now concerned. The food of the infant, its clothing, its surroundings as regards pure air, &c., ought to be the subject of the most anxious consideration. Dr. Meigs points out, by means of a very striking table obtained in Philadelphia, and presenting, in an intensified form, data which are equally well known in connection with the diarrhoeal death-rates of this country, that, in July and August, and in a less degree throughout the summer and autumn months, diarrhoeal diseases assume a prominence amounting in some cases to not much short of a hundred times as many deaths as they cause in some of the colder months: indeed, that a mean temperature above 70° is absolutely certain to bring these diseases in its train, and to a greater degree in proportion as the temperature is higher. Drs. Meigs and Pepper, therefore, recommend that, under these circumstances, children who are suffering from diarrhoeal disease should be removed as much as possible from the town into healthy and cool stations during the whole of these hot months. And this recommendation is powerfully aided by the parallel they are able to draw between the diarrhoeal diseases of infants and those observed among adult soldiers during the American war, when it was found absolutely necessary, and in fact the only effective method of treatment, to remove the soldiers affected with disease from their unwholesome surroundings in the South, and to send them to hospitals specially provided in healthy localities in the Northern States. It has also been regarded as highly expedient, in most cases, to avoid weaning a delicate child at the dangerous season of the year; \* or, if weaning is absolutely necessary

\* This very important recommendation extends back to the time of Dr. Cheyne, and is strongly corroborated in the excellent and thoroughly practical American work of Dewees, when remarking on Cheyne's statement, that "delicate children should in the autumn season be kept a month or two longer at the breast than might be thought necessary at any other, rather than be exposed to the aches and hazards which never fail to accompany this distemper" (the "weaning brash"). On this passage Dr. Dewees observes:—"This advice is in strict conformity to the usage of this country: it is so well understood by everybody here that a physician is rarely consulted upon its propriety. During the months of June, July, August, and frequently September, weaning is never performed as a matter of choice, let the age of the child be never so proper, unless it has completed the cutting of its first set of teeth. The dread of our females in this country is the "second summer."—Dewees (*On the Physical and Medical Treatment of Children* (1826), p. 331.



under such circumstances, to take extra pains to secure proper food which has not undergone decomposition, or otherwise been contaminated by noxious effluvia. The elaborate and well considered chapter upon the food of infancy in the excellent work already referred to\* may be recommended to all of you, as also the quite recently published little work of Dr. Arthur V. Meigs on *Milk Analysis and Infant Feeding*, which may be said to contain the latest data, at once scientific and practical, upon the chief food of infancy. At page 410 of Meigs and Pepper's work will be also found certain interesting statements regarding the putrescibility of milk under various atmospheric conditions, and the probability of bacterial infection as a cause of diarrhoeal disease, which, although very important, I have no time to enlarge upon. Two "broad generalisations," however, at page 418, may be here cited:—

"1st. An unhealthy food—one incompetent to furnish to the body what it needs for the purpose of nutrition, as farinaceous food or unhealthy milk, is sure to produce the disorder we are considering, no matter how favourable may be the circumstances, in all other respects, in which the child is placed.

"2nd. The best breast milk in the world, or the most correct artificial diet, will not save a child from this disorder who is located in an ill ventilated house in a dirty and filthy quarter of a large city during hot weather."

It would be difficult to place the whole subject of the hygiene of early infancy in a clearer light, or in a more comprehensive form of statement than this.

But we have seen above that, in not a few instances of disease resembling *tabes mesenterica*, it arises without much notable diarrhoea in the first instance, and certainly not as the sequence of obviously epidemic or endemic conditions. In such cases, as in all cases, it is of course necessary to treat the disease actually existing, either without reference to its antecedents, or with only such reference to these as is possible under the circumstances. Certain points may here be stated which are of quite general application. Warm underclothing and dress loosely fitting to the body should be usually adopted, unless the temperature is very high, when even the lightest and thinnest flannel is borne with difficulty, which can rarely happen in this country. But when thus protected the child should not be too closely confined to bed, or even to the house in good summer weather, as a rule, unless, indeed, the intensity

\* Meigs and Pepper, seventh edition, 1883, page 301.

of the fever, or extreme bodily weakness, or the frequency of the discharges make this imperative. Bathing in water moderately warm, and, in the great majority of cases, inunction of the abdomen with cocoa nut oil (preferable, as a rule, to cod liver oil which, however, is also often recommended for inunction), will be found very advantageous. The temperature of the bath may vary, according to the state of weakness or chilliness of the child, from 60° or 70° up to 90° or 95°. It is rare, comparatively speaking, for the mere intensity or continuity of the pyrexia to require the absolutely cold bath as an antithermic agent; but no doubt the same principles as would hold in typhoid fever may be considered as applying to the disorder now in question, under the like circumstances. My colleague, Prof. M'Call Anderson, has published several cases in which the application of ice-cloths to the abdomen as a substitute for the cold bath has, apparently, proved useful under very unpromising circumstances, both in tubercular peritonitis and in acute phthisis. The method of application is as follows:—"The night dress is pulled well up over the chest so as to avoid any possibility of its being wet, and, for a similar reason, a folded blanket is placed across the bed under the patient's body. The usual bed-clothes are arranged so that they reach up to the lower part of the chest only, which latter is covered with a separate blanket in order to prevent unnecessary exposure while the iced cloths are being changed. Two pieces of flannel are employed in the process, each being sufficiently large, when folded into four layers, to cover the whole of the front and sides of the abdomen. One of these, wrung out of iced water, is applied, while the other is left in a large basin filled with iced water at the side of the bed. The pieces of flannel are changed every minute, or so often that they still feel cold when they are removed. The changing of the flannel, especially if two persons are in attendance, one to remove the bed-clothes and the flannel, the other to apply the piece which is freshly iced, can be effected with great ease and rapidity, and without exposing the patient to any injurious extent, if the preliminary arrangement of the bed-clothes is made in the way I have indicated. I have thought it right to mention these apparently trivial details because I have often seen the process carried out in such a way as to be perfectly futile, and because I have been frequently interrogated on the subject. But I think it right to add that, in the treatment of acute phthisis, I do not wish to lay too much stress upon the value of iced cloths by themselves, but to attribute the success of the treatment to the

combination of measures employed. Of course the same precautions must be taken in the use of iced cloths as in the employment of the cold bath, and the cloths must be at once removed if there is any tendency to coldness or collapse." \* This method is certainly convenient; but it may reasonably be doubted, I think, whether too much is not claimed for it. According to all my experience hitherto, the use of cold externally applied as an antithermic relieves or palliates, it is true, the individual symptoms for the time being, but fails entirely in arresting, or even in most cases in subduing, the essential morbid process. I am not here arguing the case of hyper-pyrexia, or even of typhoid fever or pneumonia with temperatures continuously maintained for days at such a level as must necessarily prove extremely debilitating. In the affections we are now considering, this state of the temperatures is rarely present; and, while not discountenancing the use of external cold, or even ice, in such cases as may appear to require it, I am of opinion that the majority of cases do equally well or better without this. I have also frequently used quinine, and of late years antipyrine, in these cases. The latter remedy has, on the whole, answered well so far as bringing down the temperature is concerned; but its action is very fleeting, and it is extremely doubtful whether more is not lost than gained by the repeated introduction into the stomach of substances which are non-nutritious, and which, on the whole, tend to disturb digestion. It should never be forgotten that the paramount object of treatment is to preserve as far as possible the remains of the digestive and assimilative powers; and large doses of quinine, or antipyrine, or any other substance of this nature, are scarcely consistent with following out a regimen of this kind.

In dieting patients beyond the age of the first dentition, it has been a not uncommon practice on the Continent to employ raw meat. This, as a food for infants deprived of their mother's milk, or for weaned children, was recommended as long ago as 1840 by Weisse, of St. Petersburg, and has been highly praised by Trousseau and other eminent authorities on the Continent. The meat is to be cut very fine, pounded in a mortar, and strained through a sieve or cullender. The pulp, thus separated from the cellular tissue of the texture of the meat, may be rolled into small balls in salt or powdered sugar. This, given in very small quantities at first, and gradually increased, is said to arrest diarrhoeal diseases, and

\* *Clinical Lectures, &c. Op. cit., pp. 35, 36.*

to bring about restoration of health. Generally speaking, so far as my experience extends, it may be said that Continental physicians are far more inclined than those in this country to advocate a diet of meat even for very young children; and this, in circumstances under which few physicians among us would consider it suitable.\* I have no doubt that raw meat is under such circumstances found more digestible, and therefore less injurious, than some other forms of it; but, in one very striking case, under the care of Dr. Goodchild, of Bordighera, I had occasion to follow, with the interest of an intimate friend as well as a medical man, all the details of treatment under a variety of medical authorities in Paris and elsewhere for years. The case is one in which a young extremely delicate boy, now about eleven years of age, had been for a very long time subject to diarrhoeal and digestive disturbances from the very slightest errors of diet, and not unfrequently with threatenings either of chronic peritonitis or, at all events, of some disorder involving considerable swelling of the abdomen, with foul breath, enlarged papillæ of the tongue, and at times very decided feverishness. The general impression left on my mind is, that the meat diet allowed, or enjoined, under these circumstances, by foreign

\* The controversy, however, is by no means one of yesterday, or even one between British and Continental physicians, as I find from the following note in Dewees' celebrated *Treatise on the Physical and Medical Management of Children*, 1826. The author, as representing American opinion in his day, is criticising (with much appreciation, however) the essay of Dr. Cheyne, cited above:—"We were not a little surprised," he writes, "to find the following observation of Dr. Cheyne, on the subject of proper diet for children—namely, that 'an animal diet produces less irritation than one which is solely composed of vegetable matter' (p. 36). It is certainly contrary to our experience, and we had like to have said to that of everybody else. We have even found reduced milk, rennet whey, gum arabic water, thin sago, tapioca, or arrowroot, vastly more proper in all the complaints of the bowels, where nourishment may be safely permitted, than any animal juice, however much diluted. But we are persuaded that during the acute form of the disease, where purging is necessary, where there is a pain and a fever, the less the child takes into its stomach the better. . . . The only thing proper at such times is a little of the mother's milk, if the child be not weaned, or if the milk is known to agree with it, or the occasional use of thin gum arabic water." But Dr. Dewees seems to have forgotten for the moment that a diet of milk and rennet whey, &c., is *not* "solely composed of vegetable matter," such as is denounced by Cheyne. Dr. Dewees' own opinion is very much in accordance with that of his legitimate successors in Philadelphia in the present day. "During the continuance of this disease, we strictly forbid animal food or juices under any form. If the child be at the breast, let it be confined to it, if the mother has a sufficient supply; if she has not, let it be in part supported by reduced milk and a little sugar."

physicians, especially in Paris (where his parents reside) has had again and again to be abandoned in favour of a diet either absolutely, or almost absolutely, composed of milk, when the symptoms were most threatening; and, that it has been only with the greatest reserve that a return even to the lightest farinaceous articles along with milk could ever be allowed, and to meat not at all. Such, at all events, was the result in Dr. Goodchild's mind from the most careful and detailed watching of this interesting case.\* Still, in some cases, very moderate quantities of meat, or of meat juice, or still better, of meat-peptones, such as Carnrick's, or the peptonised jelly prepared by Benger, or the meat juice of Valentine, may be found useful, but only in older children.

It is very doubtful how far counter-irritation, in any extended sense of the word, should be employed, even in cases where there are physical alterations attended with considerable pain, and symptoms of inflammation locally. The addition of iodine, or of iodine ointment, to the ointment of cocoa nut oil above mentioned, is a constant resource; and, as it can be employed in every proportion, answers every purpose without the necessity of breaking the skin. Hot linseed meal poultices give relief when there is much pain, but if extended over the whole abdomen are unpleasantly heavy, and tend to produce sweating in excess. Dr. Eustace Smith recommends, accordingly, smearing the surface with a salve composed of extract of belladonna and glycerine in equal proportions, and covering this with a thick layer of cotton wool. A very old external application, recommended in the last century by Russell, in what he called "glandular tabes," is sea water; and perhaps it might be reintroduced with advantage in the form of Tidman's sea salt, used as a very strong brine warmed, and applied locally by means of flannels or spongiopiline; but I have no experience of this. Some eminent authorities recommend the application of mercurial ointment or ointments of the oleate of mercury; but this too has not entered into my practice. Opium, where either pain or intestinal irritation are present to any great degree is, according to my experience, an indispensable remedy, and may be administered with considerable freedom, due regard being had to the age of the child. Bismuth, even in large doses—*i. e.*,

\* The very last intelligence I have of this child is that he is now, after years of most anxious watching, and several winters and springs spent on the Riviera, so far recovered as to be able to vary his diet without injury, and practically well and in good bodily condition.

up to 20 or 30 grains, is sometimes very useful when there is obstinate diarrhœa. But, of course, these and all other like remedies should be limited to the critical conditions which precisely demand them. Under opposite conditions, when pain is not great and when the bowels are quiescent, cod liver oil is a valuable medicine, and when digestion is feeble, it may be aided by the pancreatic emulsion of Savory & Moore, or the *liquor pancreaticus* of Benger, which may also be given independently with good effect. Cream may be given in place of cod liver oil, or the malted cream lately introduced by Lœflund, which, however, is often found too luscious, as are, indeed, the most of the malt extracts manufactured in this country. In some cases a very little stimulant, brandy or whisky being the best, from a few drops to a teaspoonful at a time, appears to aid the digestion, restrain diarrhœa, and conduce greatly to the comfort of the patient; but it ought by no means to be converted into a routine treatment.

By these or similar means I have been led to think that the dark prognosis of what has been termed *tabes mesenterica*, and the still gloomier picture given in systematic works of tubercular and chronic peritonitis, may in some, even not a few, cases be relieved of its darkest shades. What has chiefly interfered with this conviction becoming more general than we find it among highly educated and experienced physicians, is the unconscious bias to which I have already alluded, derived from pathological anatomy. Having suffered from this bias myself in the earlier stages of my career, and only emerged from it very gradually through the clinical experiences of more than a quarter of a century, it has seemed to me a not superfluous task, even now, to lay before you the sources of the error as they appear to my own mind, and to call upon a younger generation for still further researches with respect to it. You will already have apprehended that I have no specific, and scarcely anything that can be called novel, to set forth in the way of treatment; but even well recognised facts and principles may operate in a different manner on minds prepared to receive them by sound pathological and clinical experience, as compared with those that are misled by names, and the overweening influence of great pathological authorities.

(To be continued.)

## CHRONIC LEAD-POISONING, WITH SPECIAL REFERENCE TO ITS PREVENTION.

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FOR the last two years I have been much interested in the number of cases of chronic lead-poisoning that have come under my notice among men and women, as a result of their employment in an extensive dyework in the neighbourhood. In this work the chromates of lead are much used in dyeing yarn stuffs, and in every case those affected have been engaged in that department in which the yarn is carded or combed after having been thoroughly dried, and where, in a heated atmosphere, the absorption of lead salts takes place through the pulmonary mucous membrane. The diagnosis of the first cases was to me a matter of considerable difficulty, and I am convinced that many cases of anomalous illness—such as inveterate forms of dyspepsia—coming under the care of the busy practitioner may be traced to this generally unsuspected cause. It seems that lead salts, when diffused through the air as a fine dust, are not only respired, but taken into the mouth and swallowed with the saliva, and it is an interesting fact that the symptoms of the first case were so obscure that the diagnosis was never correctly ascertained until the patient actually vomited the yellow salt. Not only so, but the diagnosis is rendered still more difficult from the facts that the patient suffers from a chronic malady, and consequently presents himself at the consulting room, and also that he almost always begins to describe his symptoms by declaring that he is “bad with his stomach.” It is acknowledged that the chief symptoms of chronic lead-poisoning are colic and paralysis, but I believe that the pernicious influences of these salts frequently express themselves differently in different constitutions, though they never go the length of producing either of these ailments. It is highly probable, also, that children who are nursed or tended by those engaged in this atmosphere, who are not scrupulously clean, may suffer in their general health, and even die from this unsuspected cause.

While admitting, then, that colic and paralysis are the chief symptoms of a well marked case of this disease, it must be remembered that in this work, where infinitesimal doses of the poison are slowly absorbed by the system, less serious consequences may at first appear. By far the greater number of my cases presented symptoms closely akin to that form of

dyspepsia in which we have severe pain during digestion, with acid eructations and sickness, but in which no amount of the usual sedative remedies had any effect in relieving. Indeed, so similar were the symptoms of these two diseases that, had it not been for the presence of the blue line along the edges of the gums, the cases must have run their course until distinct colic or paralysis had set in.

In only one case were found signs of incipient paralysis, and that was an intensely interesting one. A young woman consulted me regarding a partial loss of power, or as she described it, "a want of circulation," so bad at times that she was afraid of falling. My first impression was that this was a case of hysteria, but I soon changed my mind on her proceeding to describe her symptoms more minutely, and especially when I discovered that the extensor muscles were particularly affected, and that frequently she was unable to raise her arms until these had been rubbed or shampooed by a companion. The patient's gums presented the blue line characteristic of this disease. A most remarkable point in this case was that there was no history of indigestion, colic, or constipation.

None of the cases presented any cerebral symptoms, if we may except that of one young woman who suffered from amblyopia or general dimness of vision, which gradually disappeared under treatment.

As might be supposed, little opportunity was afforded for investigating into the condition of the kidneys of those suffering from this complaint, but in the one case examined distinct traces of the poison were found in the urine.

"As a general rule a much smaller dose will produce colic than is necessary to produce palsy" (Aitken's *Science and Practice of Medicine*, p. 845).

"Lead is an accumulative poison, and affects some persons powerfully in the smallest quantities" (Taylor's *Medical Jurisprudence*, vol. i, p. 301).

The treatment in every case consisted in the advising a restricted diet, and in the administration of sulphate of magnesium and iodide of potassium. They all recovered.

The behaviour of these drugs in removing this poison from the system is interesting and easily understood. When a soluble lead salt is introduced into the stomach, it is absorbed into the blood and carried by it to all parts of the body, and there becomes deposited. "The object of the treatment," says Dr. Lauder Brunton, "is (1) to dissolve the lead deposited in the tissues by the iodide of potassium, and to cause its elimination by the mucus of the alimentary canal; (2) to



render the lead insoluble, after it has passed into the intestine, by means of sulphate of magnesium, forming an insoluble salt (sulphate of lead); and (3) to remove it thence as quickly as possible."

Prophylaxis has been to me, however, the most serious and most important consideration. The dull, yellow, anæmic look, the depression of spirits, the pain which they suffered, and the slow return of buoyant health to these poor girls were severally calculated to awaken deep sympathy. About a year ago I brought the serious nature of the occupation under the notice of Dr. Russell, Medical Officer of Health for Glasgow, and at his suggestion one or more ventilators were fitted into this room, with so good an effect that for the following six months—so far as is known to me—none of the employees suffered from this disease. Those engaged in this department, however, seem to have no intelligent idea of the danger of breathing this noxious atmosphere, as when the cold days of winter set in this current of fresh air must have been interrupted in some way, and the result has been that lately I have had three new cases.

I have also been in correspondence with Mr. J. S. Maitland, H.M. Inspector of Factories for this district, who has not only taken a lively and intelligent interest in the subject himself, but has also obtained the co-operation of Mr. James Henderson, Superintending Inspector. In an interview I had with these gentlemen, I was asked to submit any suggestions regarding the prevention of the disease to them, and they would in turn bring the subject prominently under the notice of the proper parties.

Through the kindness of Mr. Maitland I have been supplied with a copy of the "Abstract of the White Lead Factory Act," and I would strongly recommend that the sanitary provisions thereof should be extended to dyeworks in which any of the salts of lead are used. Not only so, but it seems to me that there is even greater necessity for the application of these regulations to dyeworks of this class, inasmuch as the fine particles of the lead poison are constantly floating through an atmosphere respired by the workers, whereas in white lead works much of the labour is done in the open air or in large sheds.

One question of vast importance ought to engage the serious attention of the intelligent employer. Why is this fine lead salt so volatile? Can nothing be done to render it *fast* before it is ready for the carding room? It is a well known fact that the colouring matter is not volatile after it is

manufactured, otherwise it would be injurious to the health of the wearer. If, therefore, it could be rendered *fast* before being brought into the drying or carding room, then there would be comparative immunity from this disease. It is probable that this floating or volatile salt is unnecessary or superabundant, and in that case might be expelled by mechanical teasing and fanning, just as the microbes and dust are removed from the hair which is used by the cabinet-makers.

So long, however, as the present arrangements exist in this department, the only chance of escape from danger must lie in *perfect* ventilation. How this is to be accomplished I am at present unable to demonstrate. Certainly the means by which our churches, schools, and other public buildings are generally ventilated—viz., by gratings, windows, &c., will not suffice in this case. There must be an efficient air course by which the poisoned atmosphere is largely diluted with fresh air or totally expelled. The occupation of the coal-miner is rendered more healthy by efficient ventilation, and this at times can only be accomplished by a mechanical fan. Some such provision is just as necessary in a heated room where the workers have to wear respirators, and where, if there be not an abundant supply of fresh air, they must suffer from languor and sickness.

Under existing circumstances the employers should also supply the following:—

1. Respirators, which should be roomy, made of loosely-knitted "fluffy" wool.
2. An overall suit of compact linen.
3. Baths, hot and cold water, nail and tooth brushes.
4. A dining hall.
5. A supply of Epsom salts and sulphuric acid lemonade.

I find that these regulations are almost exactly similar to those of the White Lead Factory Act; and were these conditions and requirements fulfilled by the employers, then I would consider them neither legally nor morally liable for the consequences. It has been wisely alleged that you cannot make men *sober* by Act of Parliament, and on the same principle I am unwilling to lay the whole burden of enforcing these regulations on the employers; for, through ignorance, or the perversity of human nature, many would even "chuckle" over their infringement. From conversations I have had with these patients, I have come to the conclusion that they are, in a manner, ignorant of the danger until they are actually suffering from the poison. As a result of my investigation

into this disease and its prevention, I would suggest that certain instructions should be issued by the Inspector of Factories, and a printed copy of these presented to every one engaged in this work, and then the plea of ignorance could not be admitted.

These instructions should be somewhat similar to the following:—

“Whereas it has come to the knowledge of the Inspector of Factories that many persons engaged in dyeworks, where lead salts are used, suffer from lead poisoning, the following instructions for the prevention of this disease are hereby issued.

“I. *Cause of the Disease*—

“(a.) It may be induced by drinking water impregnated with one of the salts of lead—a very minute quantity in the gallon having been known to cause it.

“(b.) It may be absorbed through the skin by those who are handling it frequently.

“(c.) If the small particles of lead be present in the air they may get into the mouth, be swallowed with the saliva or spittle, and be absorbed by the stomach; or,

“(d.) If present in the atmosphere may be respired, and thus enter the blood through the lungs.

“Like many other poisons, certain constitutions are more easily affected by it than others, and all persons who show such liability should at once change their employment.

“Lead is known to be an accumulative poison, and hence it is quite impossible to determine the quantity or time required to produce its poisonous effects, as both the time and the dose vary in different individuals. How necessary it is, then, for all employed in such an atmosphere to be constantly on the watch for symptoms of the disease.

“II. *Symptoms of the Disease*—

“(a.) Dull yellow sallow complexion.

“(b.) Blue line along the edges of the gums immediately adjoining the teeth.

“(c.) Thirst, loss of appetite.

“(d.) Pain during the digestion of food, generally with constipation.

“(e.) Colic, severe griping pains referable to the region below the stomach, coming on at intervals, leaving more or less intense pain or uneasiness.

“(f.) Loss of power of arms and legs, blindness, wasting, death.

“These symptoms have been noted in the usual order of their appearance, but they vary greatly.

*"III. Means for the Prevention of the Disease—*

"(a.) Before entering the carding or drying room cover your mouth and nose with a respirator, and on no pretence whatever lay it aside until you have left the department. This respirator is to keep the fine dust from entering your mouth and nose.

"(b.) Cover yourself with an overall suit. Women should dress in some compact material such as linen, and men in moleskin, and these should be washed frequently. These materials are best suited for preventing the attraction of the small particles of lead.

"(c.) Be careful, while working, to stand, if possible, in a good draught of fresh air, so as to remove the foul air and dilute the remainder.

"(d.) Seldom swallow your spittle, wash your mouth frequently, and sip quantities of the lemonade supplied at the works.

"(e.) Take a small dose of Epsom salts every week whether you require it or not.

"(f.) Before leaving the work for your home be careful (1) to wash most thoroughly every part of your body that has been in the slightest manner exposed to the noxious vapour, and (2) to brush your nails and teeth. Women ought to wear their hair short, and men ought to shave. By neglecting to remove every particle of the poison from your bodies or clothes, you will not only render yourselves liable to the disease, but will be a source of danger to your friends and neighbours, and especially to any children whom you may require to nurse or tend. If all the conditions necessary for the fulfilment of these instructions be not provided by your employers, then intimation should at once be given to the Inspector for the district.

"(Signed)

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*"H.M. Inspector of Factories."*

My paper has largely exceeded the bounds I at first anticipated, but the interest of the subject and the gravity of the disease form an ample apology, and I can only express the hope that it may receive that amount of attention from all concerned that it so justly deserves.

ON THE RELATION OF THE AIR WE BREATHE TO  
OUR COMMON DISEASES.

BY FRANCIS HENDERSON, M.D.

IN view of the grand physiological conclusion, that *all the tissues breathe in the blood*, it is manifest that the quality of the air received and conveyed by the blood must have an immense influence upon health, to maintain it, to impair and destroy it, or to restore it.

That pure air is conducive to health and longevity, and that impure air is closely connected with the genesis and aggravation of disease, will be readily granted as general propositions, but it is doubtful if the wide practical importance of the subject is fully realised. Is it sufficiently considered, for example, how many of our common diseases come to us with the air we breathe? Or to what extent the prevalence of these diseases may be reduced by sanitary means?

The scientific questions involved are also of great and increasing interest, connected as they are with a department of research which has already thrown much light upon the nature and functions of the organic constituents of the atmosphere.

The following essay has no pretensions to be a full discussion of this difficult and extensive subject. It is offered merely as a contribution, and its chief purpose is to show how some recent additions to our knowledge confirm, or at least support certain conclusions bearing upon the causation of disease. With this object we shall consider—

I. The extent and nature of the relationship between the air we breathe and our common diseases.

II. The constituents of the air which are chiefly concerned in this relationship.

I. Regarding the immense influence of the inspired air in the causation of disease, very striking evidence is supplied in a recent pamphlet on *The Vital Statistics of Glasgow*.\* The author establishes, by a large body of statistics, that *house accommodation* is much the greatest factor in determining the death-rate; that the greater the number of persons living in one room, and consequently the more limited the air space, and the more impure the air, the higher the death-

\* *The Vital Statistics of Glasgow*. Jas. B. Russell, M.D., LL.D., Medical Officer of Health.

rate. These Statistics bear out this conclusion so strongly as to render of small account the other causes of increased mortality which may exist in a greater degree in these crowded dwellings, such as improper and insufficient food, insufficient clothing, &c.\*

The importance of this demonstration in a sanitary point of view can scarcely be over estimated, and as pointed out by Dr. Russell is fitted to encourage the municipal authorities in continuing the work of clearing away the most objectionable tenements, assured that even a moderate outlay in this direction will lead to still greater results in improving the health of our city.

It is, however, not so much the strictly sanitary as the etiological aspect of these results that concerns our present subject. When these results are regarded from the latter standpoint, the proofs of the disease-producing power of impure air are even more numerous; for it must be remembered that every single death generally represents a large number of cases of illness, which do not terminate fatally, but which are the products of the very same causes. In these crowded dwellings, the exhalations from living bodies mingling with the emanations which result from the action of the air upon divers sorts of putrefiable and fermentable matter adhering to the clothes, walls, furniture, &c. combine to produce a very impure atmosphere. In the Statistics of Glasgow we have now the clear broad fact demonstrated, that as these impurities become concentrated, so in like proportion does disease prevail; and conversely, that just in proportion as these impurities are diluted by the addition of purer air, so does disease diminish and the death-rate fall. Here then, in an atmosphere of this kind, we have within our grasp a mixed mass of very many of those subtle agents, which are capable of becoming the material causes of disease.

In addition to those which are material and external, we do not forget that there are other causes of disease. There are the *predisposing causes*, without the simultaneous and conjoint action of which the external material causes of our common complaints are generally incapable of exciting disease. This holds true, even when a disease, as phthisis pulmonalis, is certainly known to possess a *materies morbi* peculiar to

\* In so far as some diseases, such as the summer diarrhoea of infants, are attributable to fermentative or putrefactive changes in the food, it must not be overlooked that these are largely due *indirectly* to the condition of the air of the house.

itself. The predisposing causes are inherent to the individual; the most important are included under such titles as constitutional delicacy, hereditary tendencies to disease exhibited by the whole system or by some one organ; derangements of the constituents of the solids or fluids of the body; previous attacks of disease, exhaustion of the vital powers, &c. In regard to some of these predisposing conditions, whether acquired or hereditary, the question may be fairly raised—May they not have originated from the action at some previous time of material causes? Might the opinion not be reasonably held, for example, that a *hereditary predisposition* to rheumatism or tuberculosis is the result of the action of the material causes of these complaints upon the parent from whom the predisposition is derived. Or again, suppose an individual to be living constantly in these overcrowded abodes. For some years, perhaps, he continues in apparent health, but at length becomes the subject of some disease, the material causes of which have been always present in excess in the surrounding air. How are we to regard such a case? The most probable view appears to be that the material causes have for a long period exerted their particular influence, and have at last broken down the vital resistance of some organ or tissue, or of the blood itself. In other words, that the material agents of disease have themselves created a *predisposition*.

But we are not concerned to press this question farther at present, because, whatever may be the nature and extent of the influence of the predisposing causes, the etiological conclusions drawn from the vital statistics of Glasgow are not thereby essentially affected. For we are warranted in assuming that (apart from the considerations just suggested) the predisposing causes of disease, taken overhead, are nearly equally operative among the several populations of the different districts of the city. It follows therefore, that the action of these additional causes does not essentially modify the truth of the general conclusion, that the death-rate in the different districts is determined by the quality of the inspired air.

In endeavouring to enquire further into the relationship of the impure air of these crowded tenements to the resulting diseases, two questions suggest themselves:—

(a.) What are the particular diseases that are met with under these circumstances?

(b.) What is their relative frequency?

(a.) The Tables which are appended to *The Vital Statistics*

of Glasgow contain a considerable amount of material which, in practised hands, might supply a pretty full answer to this question, but as numerous pitfalls await the inexperienced explorer in the field of statistics, no attempt shall be made here to deal with all this material; we shall merely refer to some points which appear especially clear and prominent.

1st. *Infectious Diseases*.—One would naturally infer that from the close packing of individuals in one apartment houses, the class of infectious diseases would contribute largely to the death-rate. This we find to be the case, and it would certainly be so in a greater degree, were it not that it is in the crowded districts of the city that the sanitary authorities are chiefly engaged securing the isolation of the sick, by removal to the Fever Hospitals and effecting the proper disinfection of houses. As it is, however, when we compare, for example, the *Blythswood* district which, of the 24 districts into which Glasgow is divided, has the lowest death-rate, with the *Bridgegate and Wynds*, which has the highest, we find that infectious diseases cause 1.55 out of the total death-rate of 16.1 for 1880-1-2 in *Blythswood*, while in the *Bridgegate and Wynds* they cause 4.15 out of the total death-rate of 38.3 for 1880-1-2. Or to put it differently, of the 22.2 deaths by which the highest district total death-rate exceeds the lowest, 2.6 deaths are due to infectious diseases, the remainder, 19.6, to the non-infectious class. But it must be remembered that infectious diseases are not always prevalent. We are accustomed to regard them as invaders, attacking sometimes one and sometimes another district of the city, at irregular periods and in variable force, and therefore their material causes or contagia are not always present in the air, even of the worst sanitary districts. The statistics, therefore, of the different infectious diseases and their relative frequency may be put aside, as not bearing on our present purpose.

2nd. *Non-Infectious Diseases*.—This class contributes, as we have just seen, 19.6 out of the 22.2 deaths, by which the death-rate of the worst sanitary districts exceeds that of the best. It is therefore particularly to be noticed that it is the material causes of the *non-infectious diseases*, which so largely abound in these crowded dwellings, and chiefly swell the total death-rate of the city. Note also, in passing, that it is the material causes of these diseases, which are proved to be rendered much less hurtful simply by their dilution, or it may partly be their destruction, by purer air. In the statistical tables of the causes of death, those from non-infectious diseases are divided into the "classified" and the "unclassified,"



the latter being massed together into the "other diseases" column. These latter we shall put aside (adding to them the deaths due to *Premature Births*) as unserviceable for our present purpose, asking attention only to one important fact—viz., that the total deaths due to them also diminish in number as the sanitary conditions improve—that is to say, as the purity of the air increases.

Turning now to the consideration of the "classified diseases," we find that these include those which are the most frequent causes of death. They are our *common diseases*, and occasion 48·1 per cent of the total Glasgow death-rate.

The "classified diseases" are as follows. We place them in the order of their importance as causes of death:—

*Acute Diseases of the Lungs.*

*Consumption.*

*Nervous Diseases of Children* under 5 years. (Comprising Convulsions, Hydrocephalus, Inflammation of the Brain or its Membranes).

*Atrophy and Debility of Children* under 5 years. (Comprising Atrophy, Wasting, Want of Breast Milk, *Tubes Mesenterica*).

*Diarrhæal Diseases.* (Comprising Diarrhœa, Dysentery, Cholera, Enteritis, and Muco-Enteritis of Children).

These are the names of the common diseases, the material causes of which are present in a concentrated state in the overcrowded houses, and also exist in greater or less amount, in all the districts of the city.

Before proceeding to enquire into the relative frequency of these common diseases as a means of estimating in what proportion their several material causes are present in the air, we ask attention to some considerations regarding the *Acute Diseases of the Lungs*, which are of particular interest etiologically, because they occupy an exceptional position by contributing an undue proportion to the total death-rate of Glasgow.

According to the detailed Annual Reports of the Registrar-General for the 10 years 1875-84 inclusive, *Acute Diseases of the Lungs* have contributed 24 per cent of the total Glasgow death-rate. In Edinburgh, in spite of the prevalence of cold east winds, which are considered favourable for the development of acute affections of the lungs, these complaints in the same 10 years contributed only 19 per cent of the total death-rate of that city. In the "Mainland Rural" districts the proportion was 16 per cent. The high mortality in Glasgow from *Acute Diseases of the Lungs*

reaches its climax, as might be expected, in the worst sanitary districts, causing (according to the Glasgow Statistics for 1880-1-2) in the *Bridgegate and Wynds* 30 per cent of the total deaths. But it is to be observed—and this is the significant fact—that the Statistical Tables show that the death-rate from *Acute Diseases of the Lungs* is excessive in all, even in the best sanitary districts: proving that this abnormal prevalence is not due to the concentration of ordinary air impurities from overcrowding and other domestic insanitary conditions. Dr. Russell's conclusion is that the general atmosphere contains the material causes of these diseases in an unusual excess, and he points out that the source must be sought for in some extensive insanitary influence which includes in its range the whole area of the city. This opinion is in complete accord with the share taken by a most important factor in the production of this class of diseases—viz., cold. There is no observation more frequently made in the Health Reports of Glasgow, during the winter months, than this, that a fall in temperature is immediately followed by a rise in the death-rate from pulmonary diseases, and that, as the fall continues, so does the mortality increase.

Now, how does cold act as a cause of disease? When applied in too great a degree, or too continuously, cold lowers the vital powers, and so facilitates or *determines* the action of those extraneous materials which are, or are capable of becoming, causes of disease. This pathological principle is well illustrated by Pasteur's method for successfully inoculating fowls with anthrax. In ordinary circumstances fowls are not susceptible to this deadly virus; but Pasteur found that, by making them stand in cold water, the animal heat was lowered, and then the poison took rapid and fatal effect. Similarly it is often observed that cold determines an attack of zymotic disease in man. How often, for example, is the invasion of typhus dated from a thorough wetting, or some marked exposure to cold? In our view, the rationale of the action of cold as a cause of disease is the same whether the material is a recognised contagium or whether it is a common ingredient of ordinary air. In both cases cold acts by lessening the power of the blood and tissues to resist the action of a material which has reached them from without. The nature of the resulting disease depends chiefly on the nature of this material. By itself, cold is not capable of causing these ordinary diseases. Thus, during winter in the High Alps, with a continuously low thermometer, but with an *extra* pure air, the morbid effects of cold are not experi-

enced; diseases of the respiratory organs are seldom met with, and common catarrhs are rare. Conversely, in places where the local conditions are such that the atmosphere is generally charged with an excess of certain ingredients, it is observed that comparatively slight exposure to the action of cold is very frequently followed by the development of catarrhs, fevers, and allied diseases. On these principles, which might be illustrated and supported by many facts and arguments, it may be inferred that if a fall of temperature is immediately followed by a notable rise in the death-rate, as is the case in all the districts of Glasgow, that is a proof that the atmosphere of the whole city contains an excess of those ingredients which are capable of becoming the *materies morbi* of these complaints.\*

Again, as has been already remarked, the death-rate from *acute diseases of the lungs* is disproportionately great in the worst districts. This may be regarded as the result of the concurrence of both factors in an aggravated degree. In these districts of the city the morbid action of cold is enhanced by scanty clothing, food, and fuel, and the air impurities become even more concentrated owing to the greater care with which doors and windows are closed to retain the warmth of the house, as well as by the larger number of persons which the low temperature compels to remain indoors.

(b.) Let us next examine the relative frequency of the "classified" diseases as a means of estimating the proportion in which their several material causes are present in the air.

\* A most striking illustration of the *determining* influence of cold occurred in Glasgow in the winter of 1874-75. Dr. Russell has kindly supplied me with the statistics, which form the basis of the following statement:—

The mean temperature of the week ending 10th October, 1874, was 47° Fah. From this date, with slight variations at the first, there was a fall of the mean weekly temperature until the first week of December, when it was 38° Fah. From this point the mean weekly fall was unbroken until the first week of January, 1875, when it reached 27° Fah. Compare now the death-rate during the same period. For the week ending 10th October it was 28·3 per 1,000 living. It steadily rose, with slight variations at the first, until the first week of December, when it was 37·0. From this date the rise was unbroken, and latterly very rapid, until the first week of January, 1875, when the enormous total death-rate was reached of 69·8. Pulmonary diseases (acute and chronic), which contributed only 6·9 to the total death-rate of 28·3 in October, furnished 37·1 of the 69·8 in January. The close relationship between the death-rate and the temperature was, if possible, more markedly emphasised when the temperature rose. This was immediately followed by a great fall in the total death-rate. For the week ending 23rd January the mean temperature was 42° Fah., and a week later the total death-rate had fallen to 28·2, and the pulmonary death-rate to 10·7.

We cannot learn this from the death-rate of any one district taken by itself, because, as the mortality of the several "classified" diseases is very different, the death-rate of each does not exhibit the relative disease-rate. Thus, the deaths from acute diseases of the lungs are a comparatively small proportion of the total number of cases, while the deaths from "consumption" or "hydrocephalus" are nearly as numerous as the cases. Any attempt to meet the difficulty by calculations based on estimated relative mortality would not be satisfactory. We escape the difficulty, however, when we compare one district with another. For it is fair to assume that the death-rate from any given disease in each district is a just index of the relative amount of the material causes of that disease in the air of each district, and therefore also a just index of the relative disease-rate.

Proceeding on this basis, when we now compare the relative death-rates of the "classified" diseases in different districts, some facts of highly interesting and important etiological significance come into view. In exhibiting this comparison in the accompanying table we shall make use of the classification of the 24 districts into four groups. By so doing, the facts come out still more forcibly and clearly, because the numbers are larger and the sanitary differences greater.\*

The Table on following page shows the death-rate from each of the "classified" diseases in each group, and the point of interest to be particularly noticed is this—that each one of the classified diseases (with the exception of *Acute Diseases of the Lungs*, which we have just referred to) is seen to contribute very nearly the same percentage to the total death-rate of each group. Now, as the table exhibits the vital statistics of a very large population, extending over three years, we have a broad basis for the inference that the material causes of these "classified" diseases are not only present in the air of the four groups, but also exists in relatively the same *effective quantities* in each group.

To further accentuate this point, we may refer to the district in Glasgow with the lowest total death-rate—viz., Blyths-

\* The 24 Registration Districts into which Glasgow is divided are not well adapted to exhibit sanitary differences. The quality of the house accommodation which, as determining the amount of air space, is proved to be the main cause of difference in death-rate, is a good deal mixed. There is no district, for example, which contains houses of the best class *exclusively*. To remedy this defect as far as possible for the purpose of comparison, Dr. Russell has arranged the 24 districts into 4 groups, each group being made up of districts of nearly equal sanitary qualities.

wood. It will be observed in the Table that the "classified" diseases stand in the same order of frequency, and contribute in nearly the same proportions to the death-rate of the district as in the groups. Here then, in the *best air* of Glasgow we find the same diseases occurring in pretty nearly the same relative frequency.

TABLE SHOWING THE SIMILARITY OF PERCENTAGE OF DEATHS CONTRIBUTED BY EACH OF THE "CLASSIFIED DISEASES" TO DIFFERENT TOTAL DEATH-RATES.

*Compiled from Tables appended to the "Vital Statistics of Glasgow," 1880-1-2.*

|                       | Mean Population. | Total Death-rate per 1,000 living. | Acute Diseases of Lungs. |           | Consumption. |           | Nervous Diseases of Children. |           | Atrophy and Debility of Children. |           | Diarrhoeal Diseases. |           |
|-----------------------|------------------|------------------------------------|--------------------------|-----------|--------------|-----------|-------------------------------|-----------|-----------------------------------|-----------|----------------------|-----------|
|                       |                  |                                    | Death-rate.              | Per cent. | Death-rate.  | Per cent. | Death-rate.                   | Per cent. | Death-rate.                       | Per cent. | Death-rate.          | Per cent. |
| Group I,              | 123,661          | 19.19                              | 3.66                     | 19.1      | 2.36         | 12.3      | 1.35                          | 7.0       | .71                               | 3.7       | .42                  | 2.2       |
| "    II,              | 230,103          | 23.88                              | 5.29                     | 22.1      | 2.85         | 11.9      | 1.82                          | 7.6       | .90                               | 3.8       | .67                  | 2.8       |
| "    III,             | 75,252           | 28.73                              | 7.09                     | 24.7      | 3.47         | 12.0      | 1.82                          | 6.3       | .98                               | 3.4       | .81                  | 2.8       |
| "    IV,              | 81,827           | 31.81                              | 8.54                     | 26.8      | 3.51         | 11.0      | 1.98                          | 6.2       | 1.24                              | 3.9       | .78                  | 2.4       |
| Blythwood (District). | 26,789           | 16.1                               | 3.16                     | 19.6      | 1.99         | 12.3      | .98                           | 6.1       | .52                               | 3.2       | .40                  | 2.5       |

Now, let us suppose that the Blythwood District were separated from its surroundings, and removed fairly beyond the range of Glasgow atmosphere. The total death-rate would probably fall to some extent, and inasmuch as the diseases of the respiratory organs occur, as we have seen, in excess of the average in Glasgow air, we might expect a diminution in this class. As to what changes might take place in the relative frequency of the other "classified" diseases, we have no special statistical evidence to produce, but we know, both from general statistics and from observation, that the same common diseases would still occur—that is to say, in the *best ordinary air*.

Next, let us consider the disease-rate and consequent death-rate in *extra-ordinary air*.

Life, upon the deck of a ship at sea, in the Arabian deserts,

amid the snows of high altitudes or of northern latitudes, provided that the wants of the body as to suitable food and clothing, &c., are fully met, is notoriously free from these common diseases. Here we have no statistical proofs to offer, but the statement rests on general observation and experience, and will not be disputed. What is the cause of this freedom of disease? All are agreed that it is owing to the purity of the air. This air contains (and this has been scientifically demonstrated as regards some of these localities) much less than the ordinary amount of certain ingredients. The ingredients which are deficient are, as we shall see afterwards, just those which are in excess in places where the common (classified) diseases abound.

What are the conclusions to which these facts, considerations, and arguments lead?

That our common diseases are dependent for their existence upon causes which are material.

That this material is present in large quantity in the air of crowded dwellings, and occasions the prevalence of these diseases in such localities.

That the same material exists normally to a certain amount in the best ordinary air, and is capable of becoming the material causes of these diseases. This does not imply that the ingredients of normal air are concerned in the first step or steps of the departures from health which are the beginnings of disease (these have their origin in some failure or breakdown of the natural arrangements in the body itself); but it does imply that the presence and action of certain air ingredients are necessary for the complete disease-process.

Further, the above conclusions do not imply that the matter from without which becomes the material causes of our common diseases can only possibly reach our bodies with the air we breathe. It may, and probably does regularly enter with the water we drink, which, however, is the only other medium absolutely common to all.\* In the case of the Glasgow Statistics, however, this consideration may be put aside, because the water supply is of *uniform quality* in all the districts of the city, and therefore it has no share whatever in producing the different death-rates in these districts. Again, it might be argued that as the Glasgow water must be admitted, in respect of purity, to be at least equal to the best ordinary standard, it follows that the amount by which the

\* Some might add *food*, practically, however, the material causes which food universally and normally conveys to us are derived directly or indirectly from the air or water.

(non-infectious diseases) death-rate of the "best Glasgow air" exceeds the death-rate of the "best ordinary air," is not due to water ingredients.

These, or similar views of the relation of the air we breathe to our common diseases, are not new; they have been expressed more or less explicitly by many writers, but they have not hitherto obtained that attention which they deserve, being generally regarded as merely theoretical opinions of little practical value. But when the subject is approached from the statistical side, when we travel upwards through a series of decreasing death-rates, in the production of which the sole constant agent or factor is proved to be a larger and larger supply of purer and purer air, or, in other words, a greater and greater diminution of certain ingredients in the air, until we finally reach localities characterised alike by the unusual (abnormal) rarity of ordinary diseases, and by the unusual scarcity of these same air ingredients, then the conclusion that these diseases are dependent on material causes, and that the necessary material exists in ordinary air, is a logical deduction from established or at least admitted premises.

The establishment of this conclusion is chiefly due to the statistical method of enquiry. No individual probably could have established this relationship from his own personal observation. No doubt the belief has long prevailed that the atmosphere of crowded abodes was unhealthy, but the reasons of the opinion were ill defined. When these common diseases were observed largely to abound in impure air, the conclusion that some ingredient of the air itself stood in causal relationship, in any sense, seemed to be negatived by the fact that the same forms of disease were regularly met with amid the best sanitary surroundings, in what was esteemed pure air. We now know that the explanation of the apparent anomaly is this, that the best ordinary air contains a certain amount of ingredients which can supply the material causes of these diseases. \*

This being so, we naturally infer, that the excessive prevalence of our common disease is due to a concentration of

\* In connection with these views, it is of particular interest to note the conclusions which are arrived at by Prof. Hirsch in his great work on *Geographical and Historical Pathology*, regarding the distribution of phthisis. He finds that phthisis, the material cause of which is now known to be a specific form of matter essential to the production of the disease, is most prevalent in large centres of population in connection with overcrowding and impure air, but it is to be met with *everywhere*; it is a disease of all times, all countries, and all races.

these same air ingredients. Such a concentration is the natural and normal result, where the processes of human life, and the necessarily associated processes, are carried on in a limited air space. Under these circumstances, each one of our common diseases rises in prevalence, maintaining, however, in so doing, much the same relation to the others as regards frequency. This is a most interesting phenomenon, and requires further study. It implies that a very intimate relationship subsists between some particular, although common, air ingredient and each of our common diseases.

These considerations furnish a valuable guide to the sanitarian. If, in any house, tenement, or locality, he finds an excessive prevalence of all the common diseases in pretty much the ordinary proportion, this indicates that there is no special or unusual source of the causes of disease, but that there is a deficiency of the air supply in proportion to the ingredients which are being discharged into the air. On the other hand, if he finds that some one form of (non-infectious) disease prevails, or that one class of disease is asserting itself prominently, then he infers that there is some special source of air contamination affecting either the domestic or the general atmosphere. Thus, in the case of Glasgow, for example, the Vital Statistics show that the acute pulmonary diseases contribute quite an undue proportion to the common disease death-rate. The conclusion then is, that the excessive quantity of the material causes of this class of disease is supplied from some special source, and this conclusion is confirmed by the following statistical facts:—The sanitary operations which have been effectual during the last ten years in decidedly reducing the death-rate of all the other non-infectious complaints, have had scarcely any appreciable influence upon the abnormal prevalence of acute pulmonary diseases. Then, there is this other fact, that every district of the city suffers an excessive mortality from the acute pulmonary diseases, proving that it is the general, and not the domestic atmosphere, which is at fault.

The very great practical importance of this subject is obvious to all, but it receives definite and forcible illustration in *The Vital Statistics of Glasgow*, from the details of which we learn what might be accomplished in reducing the general mortality of our common diseases. There clear evidence is supplied that effective sanitation as regards Glasgow would have a far greater influence upon the death-rate due to the common *non-infectious* diseases, than it has had, or even, if carried out in the most perfect manner, could have, upon the



death-rate of the *infectious* class. For this reason, that the former class of disease occasions more than three times the number of *preventible deaths*. \* There is, however, a notable distinction between the results of sanitation upon these two classes of disease. The *infectious* complaints, or at least some of them, may be entirely abolished or "stamped out," as Dr. Russell claims has been practically accomplished in Glasgow, in the case of small-pox and typhus. But, although the common *non-infectious* diseases can be immensely reduced in prevalence, they can never be abolished by sanitary operations, because their material agents are normally present in normal air.

We shall now proceed to consider, what are the constituents of the air, which are concerned in its relationship to our common diseases.

(To be continued.)

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## CURRENT TOPICS.

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SIR GEORGE H. B. MACLEOD.—The profession in the West of Scotland has been much gratified at the honour bestowed on one of its most prominent members, by the knighting of Prof. Macleod. His position and attainments well entitle him to the honour. A certain feeling of disappointment has mingled with the gratification in the minds of many, that a similar distinction was not conferred on the Physician to the Queen and Professor of Medicine. It may alleviate this feeling if we state that it is an open secret that Dr. Gairdner has by his own choice retained his simple professional designation. This being so it must be felt that this decision does great honour to the man, and that by his action in placing purely Professional Distinction above Titled Rank he has done honour to the Profession as a whole. At the same time there are some who do not share his opinion, and would have been pleased to have seen him alongside of his colleague.

GLASGOW MEDICAL CHARITIES COMMITTEE.—This committee held its usual monthly meeting in the Faculty Hall on 26th

\* If we take the lowest district death-rate from *infectious* and from *non-infectious* diseases as an attainable standard for the whole city of Glasgow, the difference between it and the present death-rate of the whole city will afford an estimate of the *preventible deaths* from each class.

May, Dr. James Morton in the chair. The work done so far has been of a most important nature, although the details of all the business have not yet reached that stage when a full and elaborate report of the different methods of discrimination exercised by the managers of the hospitals throughout the country, will be laid before all interested in our hospitals. A sub-committee has been diligently employed looking into the information collected by the Secretary from upwards of 100 different sources, and as soon as the work has been completed it will be laid before the general committee for its consideration. The Secretary of the Charity Organisation Society has rendered the committee most valuable assistance, and at a meeting of the medical relief committee of that Society, it was resolved to have a conference with the Medical Charities Committee in order to obtain their assistance, advice, and co-operation, so that a satisfactory system of affording medical relief to the deserving poor at all times should exist. Valuable information, bearing on the abuse of medical charities, and the methods adopted by the Parochial Boards for preventing it, has been received by the Secretary of the committee.

At a meeting of the Charity Organisation Society, it was resolved to request the various medical charitable agencies in the city to render assistance to the families of the able-bodied poor whom the Society has been assisting at its own expense for upwards of a year by reason of the order from the Board of Supervision in Edinburgh to the inspectors of the poor, prohibiting them to afford medical relief to such poor people. The Charity Organisation Society appointed a medical practitioner to attend to these cases, and pays him for his services and for the medicines he supplies. The Society, however, considers that as the care of such suffering poor persons was only undertaken by it in exceptional circumstances, and as it exists for the great object of organising charity, the valuable medical charities in Glasgow only require to be made aware of this good work the Society has been doing in order that they may gladly and more properly take it up themselves. Such an agency, for example, as the Glasgow Medical Mission is admirably adapted for affording suitable attention to that class of the poor. The cases recommended for treatment will be carefully investigated by the Charity Organisation Society, all cases receiving medical relief at once on presenting themselves for the first time, the merits of the applicants being obtained afterwards. The Council of the Charity Organisation Society hope that all the medical charitable agencies in the city will soon see their way

to avail themselves of the staff and machinery of the Society for the purpose of enabling them to administer the help which they afford in the best interests of all concerned. It is the desire of the Council of the Society to bring about harmonious action between the institutions giving medical relief, the Charity Organisation Society, and the Parochial Boards, so that overlapping and wastefulness of resources may be minimised, and gratuitous relief given only to those who have no legal claim on the parish and are unable to pay.

It was arranged to hold a conference at an early date with the Medical Charities Committee appointed at the recent public meeting of practitioners in the Faculty Hall.

In pursuance of the above arrangement, the Medical Charities Committee met in conference with the Medical Committee of the Charity Organisation Society, in the rooms of the latter at 115 Bath Street, on Thursday, the 23rd ult. Various suggestions were made towards obtaining the co-operation of all who have at heart the best interests of the medical charities of the city. It was finally resolved to hold a conference with the members of the boards of management of these institutions, and of the parochial boards, for the purpose of discussing any points connected with the abuse of medical charity which may require elucidation, and for appointing a final and representative committee to take the most advisable steps in the way of instituting a definite and practicable system of restricting gratuitous aid to the needy and deserving poor. The Secretary of the Medical Charities Committee reported that a statement would soon be submitted, showing the means adopted in various parts of the country to check or prevent charity abuse. The Committee expressed thanks to the Charity Organisation Society for their kindness in affording an opportunity for conference, and for their sympathy and co-operation.

**THE ANTISEPTIC TREATMENT OF PHTHISIS.**—Since the general acceptance of Koch's discovery of the tubercular bacillus and the recognition of the action of this microbe in the pathogenesis of phthisis pulmonalis, it has occurred to many minds to look for some means of treatment which would aim at destroying the vitality of the microbe. A practical advance in this direction is signalled by the issue of a "Bulletin du laboratoire de recherches expérimentales et cliniques sur la traitement aseptique de la phthisie pulmonaire," the second number of which has been recently issued. The bulletin consists of a pamphlet of 140 pages, and it is intended

to come out quarterly. The authors have endeavoured to establish their treatment on a firm scientific basis, and, with this view, have studied the effects of various agents on the growth of the tubercular bacillus under cultivation. They have availed themselves of the material devised by Nocard and Roux, which is a much better medium for cultivating this organism than Koch's well known jelly. In order to test the effects of agents, they have mixed them with Nocard and Roux's jelly, and watched their effects in rendering the medium more or less unfertile. The substances tested in this way were carbolic acid, sulphite of sodium, chloroform, iodoform, thymol, eucalyptol, and aniline oil, the general result being that pure phenic (carbolic) acid is the most efficient, while sulphite of sodium stands next. When the very fertile medium of cultivation already mentioned is used, carbolic acid is the only antiseptic which succeeded in rendering it unsuitable to the evolution of the tubercular bacillus when present in the proportion of about 3 per cent.

Having determined that carbolic acid is the best antiseptic for the bacillus, the authors have proceeded to consider the best means of introducing this agent into the body in sufficient quantity to saturate the tissues, and render them unfertile so far as this microbe is concerned. Administration by the stomach, rectum, and lungs, is considered, but all of these are much less convenient and exact than the hypodermic method. The authors assert that hypodermic injection, if done with due precautions, is much better borne than has been generally supposed. They say, "Provided that we conform to certain very simple rules in regard to the manual operation, and that we scrupulously observe the elementary antiseptic precautions, the skin accepts without protest, or, at least, without appreciable disturbance, substances considered the most irritant. As to the quantities of fluid injected, we are able, without inconvenience, to inject under the skin distilled water and indifferent fluids, in quantities not only measured by drops, but by grammes, or dozens, or even hundreds of grammes." Some of the precautions noted are, not to insert the needle obliquely but perpendicularly, so as to pierce the whole skin, and to do so with a single stroke. The places chosen for injection by preference are the flat part of the buttock, immediately behind the trochanter major, and the trunk in front, or behind, more particularly at the sides. The syringe and needles should always be carefully sterilised, and the needles should be frequently sharpened on a hone, as bluntness is a great cause of pain to the patient.

The authors give us the formula for the various solutions which they use. The watery solutions are of the strength of one per cent and two per cent. They have also solutions of olive oil thoroughly sterilised 50, liquid vaseline 40 or 30, and absolute phenol 10 or 20, according as a solution of 10 or 20 per cent is wanted. The advantage of the oily solution is that it is more slowly absorbed, and the doses can be given less frequently. By these solutions, and more particularly by the oily one, considerable quantities of carbolic acid are introduced into the system. The dose of the watery solutions varies according to the indications of the case, from 10 cubic centimetres a-week, to 5, 10, 15, 20, or even 25 a-day; the average being about 5 every two days or every day. Of the oily solutions, doses of 200 drops of the weaker, and 100 of the stronger, may be given. The authors give the results of treatment in 122 cases, which show a respectable proportion of cures and improvements.

Certainly we have here a very suggestive piece of work, and there is at least distinct encouragement to go on in the same line in the hope of securing useful results.

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## REVIEWS.

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*Neurectasy; or, Nerve-Stretching for the Relief or Cure of Pain: being the Bradshaw Lecture for 1883, with Appendix by the Author, dated March, 1887.* By JOHN MARSHALL, F.R.S., F.R.C.S., &c. London: Smith, Elder & Co. 1887.

THOUGH the "deformed creature, Fashion," has less influence in surgical science than in most other sublunary matters, it undoubtedly has an effect, both important and (we think) often injurious. If we are asked for proof of this, we would refer to the unreasonable use of nerve-stretching some five years ago for nervous conditions the most varied, and where the chance of doing any good was most problematical, and would compare the craze of that date with the equally unreasonable neglect of this mode of treatment in the present year of grace. It is certainly curious that Mr. Marshall's admirable Bradshaw lecture, delivered at the height of the nerve-stretching fever, should only be published in book form now, when the operation has largely fallen into disuetude.

Certainly, the author is now able to review more coolly and judicially, than was possible in 1883, the evidence as to the effect of nerve-stretching in the various forms of nerve affection in which it has been tried.

It has long been our conviction that neurectasy is by a long way more successful in neuralgic affections than in other forms of nerve disease, and we might even go farther, and say that its success is in direct proportion to the intensity of the neuralgic pain. We are glad to see that Mr. Marshall's experience, as well as the collected experience of other operators, leads to almost precisely the same result. He says—"It is certain, first, that nerve-stretching has established itself as an operation capable of both a curative and palliative action in a large majority of cases of neuralgia; and it must be admitted that a return of the disease after the interval of a certain duration, say of one year, ought sometimes to be regarded as an entirely fresh attack." The result of the operation in cases of disease affecting the nerve centres he affirms to be "palpably unhappy or, it may be said, disastrous; the few tabulated cures (about 2 per cent in myelitis and ataxy taken together) may be regarded as apocryphal; the instances of amelioration, it is to be feared, are more imposing numerically than actually, most of them being admitted to be only temporary; the failures are pronounced, and the deaths attributable to the operation are numerous."

Those who read the lecture when it was first published in the medical journals will remember that Mr. Marshall suggested a theoretic explanation of the advantage derived from nerve stretching. This was that there existed, within the nerve sheaths, sensory nerve endings or *nervi nervorum*; that when a nerve underwent inflammation the inflammatory products pressed on these terminal filaments and caused pain, which was not referred to the peripheral distribution of the main nerve. When the nerve is forcibly stretched, he argued, these *nervi nervorum* are stretched, or even broken across, and paralysed, and the pain experienced from them is consequently abolished. Now, at the time the author broached this theory, such nerves had only been demonstrated as existing in the optic nerve; but he ventured on a prophecy that true sensory nerves would be found in the epineurium or perineurium of all nerves. We can excuse, therefore, the air of triumph with which he points out that a few weeks after his lecture was delivered the existence of such nerves was demonstrated by Mr. Victor Horsley.

Of nerve-stretching in sciatica the author speaks more

favourably than most surgeons will consider justifiable; we are, however, satisfied that he has good grounds for his statement. In our own experience we have met with no case of sciatica which has not been benefited by the operation—and in several instances we have seen the patients after an interval of several years, and have ascertained that they have had no return of the disease.

While advising that the non-cutting operation should be tried, as being simple and safe, he has no confidence in its efficacy, but acknowledges that he “has had but one experience of the bloodless operation, and that not a very satisfactory one.”

We trust that the publication of Mr. Marshall's lecture, with the added appendix, will induce many surgeons to give a new trial to neurectasy, especially in neuralgia, and have no doubt that the operation is even yet destined to take a permanent place in the treatment of this painful and intractable form of disease.

*A Junior Course of Practical Zoology.* By PROFESSOR A. MILNES MARSHALL and C. H. HURST. London. 1887.

SINCE 1870, when Rolleston published *Forms of Animal Life*, various attempts have been made to supply students of Zoology with serviceable books for laboratory work. Huxley and Martin's small work, describing selected types of plants and animals, was supplemented three years ago by T. J. Parker's *Zootomy*. While the former of these was lacking in vertebrate forms, a description of one only—the frog—being given, the latter was confined to vertebrates, considering examples from all the chief vertebrate groups except the Amphibians. The joint author with Huxley of *Elementary Biology* published in America, since his appointment to Johns Hopkins University, a series of handbooks on *How to Dissect a Chelonian*, and other vertebrate forms easily procurable on the Atlantic sea board of America. Besides these, we have had useful monographs: as Krause, *Die Anatomie des Kaninchens*; Ecker, *Die Anatomie des Frosches*; Huxley on the *Crayfish*; Mivart, and Wilder and Gage, on the *Cat*. But with all these, it was still felt that we lacked a really good work to enable beginners to obtain a practical acquaintance with the leading facts of animal morphology. Professor Marshall and Mr. Hurst have now furnished us with the most complete and satisfactory text-book of any yet published on Practical Zoology. In *Practical Zoology* are described the following

forms from the Metazoa:—Hydra, liver fluke, leech, earthworm, freshwater mussel, snail, crayfish, cockroach, lancelet, dogfish, pigeon, and rabbit. The value of the book would certainly have been enhanced if the authors, instead of dealing with two so closely allied forms as the leech and earthworm, either of which would amply illustrate annulate worms, had devoted the space allotted to one to a description of one of the Echinoderms—*e. g.*, the star-fish. Perhaps, too, even though the senior author had previously written a small monograph on the frog, this typical and easily obtainable vertebrate might have been dealt with without making *Practical Zoology* too cumbersome or too expensive.

It must always be a difficulty with the authors of a work of this kind what to omit and what to select. Undoubtedly their discretion has been wisely exercised in preferring the lancelet to the ascidian, as one cannot easily obtain the tailed adult or the tailed embryo of the fixed ascidian. In the description of Hydra, the very thin homogeneous layer between the ectoderm and the endoderm has been called mesoderm, in opposition to the views held among zoologists. That the authors have illustrated trematodes by the liver fluke of the sheep is perhaps natural, from its large size, but a handier way of obtaining specimens of these entoparasites, and one that would yield even better results on application of ordinary clearing reagents, would be to take the species inhabiting the alimentary canal or lung of a frog. The leading facts in the life history of the liver fluke have been appended, but the authors seem to dissuade the laboratory student from verifying the history by stating that this can only be studied under very special conditions. The facts can easily be tested by taking the eggs from the bile ducts of infected sheep and hatching them. The eggs may be kept in an arrested state of development in an incubator in the dark, and when removed to the light we obtain the free swimming embryo, which bores its way into a gasteropod *Limnæa truncatula*, where it becomes the Sporocyst containing the Rediæ. Leuckart surmised and Thomas proved the development of the free swimming form into the Sporocyst. Sporocysts, Rediæ, and the following stage—the tailed and active Cercariæ, can be obtained from most individuals of the *Limnæa truncatula* very easily. The Cercariæ are shed from the gasteropod, and become encysted on grass or in water, and are taken up by the sheep, giving rise to the liver-rot.

The arthropods described are very well done, the description



of the appendages being very fully given. The instructions to the dissector are clearly set forth, being distinguished from the descriptive portion by being italicised.

In describing the skull of the rabbit, the indication of the membrane bones by italics, and of the cartilage bones in Roman type, is an advantage to the student.

The drawings, which sparingly illustrate the letterpress, will be of great assistance to the dissector, and help to wean him from the too great reliance which he often places on his atlas, itself not always accurate in detail, and thereby assist in removing the temptation which besets him in taking for granted that certain structures are present, although he has never seen them for himself. The cross sections introduce the junior student to the interpretation and construction of complete organs from successive sections, which is now the chief method of embryology.

The chapter on the use and manufacture of reagents, and the insisting on making drawings of all dissections, preface a work which will be mutually helpful to the teacher of comparative anatomy, and to those who seek an elementary acquaintance with the great facts of animal morphology.

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*The Treatment of Spina Bifida by a New Method.* By JAMES MORTON, M.D., L.R.C.S.Ed., Professor of Materia Medica, Anderson's College; lately Surgeon and Clinical Lecturer on Surgery in the Glasgow Royal Infirmary; President of the Faculty of Physicians and Surgeons of Glasgow. With a Paper on the Pathology of Spina Bifida, by DR. JOHN CLELAND, Professor of Anatomy in the University of Glasgow. London: J. & A. Churchill. 1887.

It is now ten years since Dr. Morton published the first edition of this book, and astonished surgeons by the very large measure of success he had obtained in the treatment of spina bifida by the injection of his iodo-glycerine solution. During these ten years Dr. Morton has added largely to his own experience, and other surgeons have adopted his suggestion, and a large mass of evidence is at hand to testify to the value of the method. The subject attracted a good deal of attention at the Clinical Society of London, where one and another case, which had been successfully dealt with in this way, was reported. The matter was felt to be of so much importance that the President nominated a committee of four surgeons to investigate it. This committee reported some time ago, and Dr. Morton has availed himself largely of this report in

the preparation of the second edition of his work. The book has grown a great deal, and Dr. Morton now deals with the whole question of spina bifida more fully than before, and at the same time quotes a large number of cases in which his method of treatment has been successful. In the main, his opinion coincides with that of the committee of the Clinical Society, but he inclines to adopt the treatment earlier, and also in more desperate cases than the members of the committee recommend.

These are points fairly open to argument; Dr. Morton looks at them as an enthusiast, and his faith is probably greater than that of the general body of surgeons. There is certainly much to be urged in favour of the more cautious views of the committee.

Dr. Cleland's contribution to this work is of extreme interest, and ought to attract general attention. He indicates a serious hiatus in the pathological section of the committee's report, and deals with his part of the subject in a masterly manner. His main contention is that the spinal cord plays a much more important part in this deformity than has been up to this time admitted. He connects all forms of spina bifida with syringocele or dropsy of the central canal of the cord. This may be so extreme as to lead to the rupture of the cord, and the formation of an open spina bifida along a part or the whole of the cord. Cases of closed or tumescent spina bifida he groups into metaneural with the effusion behind the nerve-roots, and perineural where the effusion is around the nerve-roots, and in both groups he finds evidence of syringocele. Dr. Cleland's paper is as welcome a contribution to the pathology of the deformity as Dr. Morton's work has been to its treatment, and both authors are to be congratulated.

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*Subjective Symptoms in Eye Diseases: being Chapters on the Disorders of Vision Symptomatic of Diseases in the Eye and Central Nervous System.* By GEORGE A. BERRY, M.B. Edinburgh: Oliver & Boyd. 1886.

THIS volume consists of a number of papers originally published in the pages of the *Edinburgh Medical Journal*. As they treat of such subjects as colour blindness, metamorphopsia, night blindness, diplopia, polyopia, &c., and are thoughtfully and carefully written, they will repay perusal by those who make the diseases of the eye a special study. They will also prove instructive to physicians, who not unfrequently find

the subjective phenomena herein discussed troublesome both in relation to diagnosis and treatment.

In his opening paper Mr. Berry makes a much needed protest against the tendency of the younger ophthalmologists to regard the affections of the eye too exclusively from the objective standpoint. Some of them have such confidence in objective methods, that they will order glasses for a case of mixed astigmatism from their examination with the refraction-ophthalmoscope or shadow-test alone, to find, perhaps, that when the patient returns with the glasses, he complains that he cannot use them. "There are," he justly observes, "sources of error which render the result of a determination of this nature uncertain, so that even the most practised observer will be found on trial to make mistakes, not only in estimating the higher, but also the lower degrees of ametropia."

Mr. Berry does not aim at presenting anything original or novel, but rather endeavours to set forth, in plain language, facts regarding subjective phenomena which are either overlooked or receive insufficient exposition, even in the special treatises on diseases of the eye.

We may especially commend to the attention of our readers the chapter on photopsia and chromotopsia, as containing much interesting matter. The occurrence of red vision after cataract extraction was noticed in one case under his own observation, and also in another patient, who had a coloboma of the iris without cataract extraction. The author concurs with Benson and Purtscher in regarding the phenomenon as due to hyperæsthesia of the retina, but adds, "Whether the condition of the retina is favoured by the aphakia and the presence of a coloboma of the iris, or is brought about in some other way is not clear."

These essays are a valuable contribution to the literature of the subject, and are a credit to the British School of Ophthalmology.

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*Insomnia and other Disorders of Sleep.* By HENRY M. LYMAN, M.A., M.D., Professor of Physiology and of Diseases of the Nervous System in Rush Medical College, &c., &c. Chicago. 1885. Pp. 239.

THE first chapter of this volume is a purely physiological one, and is devoted to the nature and cause of sleep. Unfortunately, it sheds no new light on this somewhat obscure department of physiology. The rest of the book treats of dreams, somnambulism, and hypnotism, with a chapter on the remedies for

insomnia. The style of the writer is pleasing, and, as a rule, what he has to say is well said; still, there is a want of originality in the matter, which makes the book of less interest than it would have been had the case been otherwise. The only part likely to be of value to the practitioner is the chapter on the treatment of insomnia, which is pretty full, and generally accurate, though exception might easily be taken to the course of treatment recommended for insomnia due to renal disease and some other conditions, while most will be cautious about employing the doses recommended in some cases. The volume will repay perusal.

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*A Treatise on the Science and Practice of Midwifery.* By W. S. PLAYFAIR, M.D., &c., Professor of Obstetric Medicine in King's College, &c. Two Vols. Sixth Edition. Pp. 411 and 424.

IN the number of this *Journal* for August, 1886, our readers will find a review of the last edition of this work. That now published is almost a literal reprint of the last, so that little more need be said either for or against it. The only noteworthy additions are two plates after H. T. Barbour, one a section of a uterus after death from hæmorrhage during labour, the other a vertical section of the pelvis with *post-partum* uterus.

The fact that this treatise has run through six editions in twelve years speaks well for its acceptance with the professional public, and we still feel disposed to speak of it as one of the best books on midwifery.

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*The Management of Labour and of the Lying-in Period: a Guide for the Young Practitioner.* By HENRY J. LANDIS, A.M., M.D., Professor of Obstetrics and Diseases of Women, Starling Medical College. London: Charles Griffin & Co. Pp. 334.

THIS volume, as its title page indicates, has been got up for the benefit of those who, having neglected the opportunities for the practical study of obstetrics during their university course, meet with difficulties on the threshold of their public career. Unfortunately, it cannot take the place of practical experience, yet so far as a book can afford assistance, it does so. Every difficulty likely to be met with in the practice of obstetrics is fully and ably dealt with, the directions for operative procedure being clear and without ambiguity. The

nature of the subject necessarily renders such a work more a catalogue of methods than a scientifically worked out treatise, yet the author has contrived to infuse a considerable amount of interest into somewhat uninviting matter. The style is good, though frequently marred by Americanisms which sound strangely and somewhat slangy in British ears, and by an indulgence in the nineteenth-century craze for the manufacture of new words. We have all heard of Credé, but the phrase, "the uterus should be Credéd," looks very un-English.

This handy work fully accomplishes the object kept in view by its author, and is likely to be found of great value by the young practitioner when face to face with a labour difficulty.

*L'Enseignement Actuel de l'Hygiène dans les Facultés de Médecine de l'Europe.* Le DR. LOWENTHAL. Pp. 126.

As the title indicates, this little work is meant to show to what extent hygiene is actually taught in European medical schools. It is satisfactory to learn that a considerable amount of time is now devoted to this subject in Spain and some other countries equally backward in practical hygiene. The author recommends the teaching of the elements of hygiene to all medical students towards the end of their curriculum, and the giving of special courses of lectures to non-medical students, sanitary inspectors, and so on, and more particularly to future professors of hygiene. It is somewhat strange that, while preventive medicine is acknowledged to be of the utmost importance, there should be no course specially devoted to public health in the *ordinary* curriculum for medical students in this University.

*Hysterie et Traumatisme. Paralysies, Contractures, Arthralgies, Hystero-traumatiques.* DR. P. BERBEZ. Pp. 127.

In this work the author attempts to prove that hysteria is a distinct morbid entity. He gives a careful account of paralysees, with or without contractures, having injuries for their exciting cause. He enters fully into the pathology and symptoms of such cases, and gives the essential points in the diagnosis which, he says, can always be made. The phenomena are due to *auto-suggestion*, and are identical with those induced in hypnotic subjects by suggestion. The illustrative cases are well recorded, and will prove of interest to neurologists, whether or not they agree with the author's conclusions.

**MEETINGS OF SOCIETIES.****MEDICO-CHIRURGICAL SOCIETY.**

SESSION 1886-87.

MEETING IX.—4TH FEBRUARY, 1887.

**I.—ON HÆMORRHAGES FROM THE UNIMPREGNATED UTERUS.**

BY DR. JAMES STIRTON. (See page 1.)

Dr. Stirton delivered his introductory address as Vice-President of the Society—Obstetrical and Gynæcological Section.

On the motion of Dr. Wood Smith, a hearty vote of thanks was accorded to Dr. Stirton for his address.

**II.—EXTRA-UTERINE PREGNANCY.**

BY MR. J. T. CARTER.

Frozen sections of a female who died during Extra-Uterine Pregnancy were shown, and the following remarks were made on the case:—

The sections which I bring before you to-night were obtained from a female subject, aged 38 years, which was brought into the dissecting room, having the usual appearances of pregnancy. The body having been frozen, it was carefully sawn into six slabs, each about  $1\frac{1}{2}$  inches in thickness, which were embedded in plaster of Paris, and preserved in methylated spirit. After freezing, the case was discovered to be one of extra-uterine pregnancy, and has so many points of interest that I propose simply to-night to draw your attention to a few of the more important, as the whole subject will be fully gone into at some future date. Confining ourselves to what is seen in the abdominal cavity, our attention is first drawn to the kind of extra-uterine pregnancy; and taking Lawson Tait's simple classification as our basis, it may be said to be tubal in character. The foetus and its capsule are extra-peritoneal, the former being jammed in the cavity of the pelvis between the uterus and posterior pelvic wall. This will, no doubt, account for the great antero-posterior flattening of both the thoracic and abdominal cavities, the extremities being twisted so that they rest in a concavity

formed by the greatly extended head and body of the foetus. The placenta consisted of a large oval shaped mass adherent to the wall of the cyst; the umbilical cord passing from the foetus, and being attached to its upper portion. The uterus reached upwards for about 4½ inches above the pubes, and the peritoneum was stripped from its posterior wall, fundus, and upper part of its anterior surface. Other points of interest to which I would like to draw your attention, are the height of the tumour and diaphragm, and the thickness of the left ventricular wall of the heart, although this is not a case of normal pregnancy.

The sections well illustrate difficulties which would be encountered in any attempt to relieve the mother by the operation of gastrotomy. The height of the enlarged uterus, together with its attachment to the sac of the cyst above, would necessitate a very large abdominal incision. Obstacles would be encountered in the extraction of the child from its depth in the pelvic cavity, together with the size and weight of the enlarged placenta above.

*Dr. W. L. Reid* said that, with regard to not finding the decidua, it was usually about the third month that the uterus cleared itself. He had on two or three occasions verified that the uterus did not return to its former position.

*Dr. Samuel Sloan* said that he had measured the uterus in *Mr. Carter's* case, and found it only very slightly enlarged. He asked *Mr. Carter* to say whether the placenta was thickened to any extent, and also what was the condition of the soft parts of the foetus.

*Mr. Carter* said that the placenta was greatly thickened. The foetus was perfectly fresh. *Parry* and *Deschamps* mentioned cases in which children had been taken out after a period of ten years, perfectly fresh in condition.

### III. — A NEW UTERINE DILATOR FOR USE IN GYNÆCOLOGICAL CASES.

BY *DR. W. L. REID.* (See p. 345, vol. xxvii.)

*Dr. Turner* said that he had tried both the dilator which *Dr. Reid* had invented, and that of *Dr. Samuel Sloan*, also exhibited that evening, and could testify to the efficiency of both these instruments. *Dr. Sloan* had a urethral dilator constructed on the same principle, and this he had tried with great benefit. The perfect gradation of the dilators greatly facilitated their use. He could bear testimony to the ease with which *Dr. Reid's* instrument could be introduced, and

when introduced, the operator could easily draw down the uterus. There was no damage to the tissues following its use, as far as he had observed.

*Dr. S. Sloan* said that *Dr. Reid's* instrument was ingenious, but he would fear that the screw would damage the tissues. How would the operator know that the point was proceeding in the axis of the uterine canal? *Dr. Reid* had stated that in half the cases the instrument was used without effect. Did he mean without effect as regards the cure of sterility or of dysmenorrhœa? He would also ask a statement of what quantity of blood was lost?

*Dr. Murdoch Cameron* said that after ascertaining that the particular instrument exhibited by *Dr. Reid* had not been used, he introduced it into his mouth, and found that it had no control on the mucous membrane of the mouth. He would prefer to use *Mr. Hilliard's* instrument, which *Dr. Reid* had also shown that evening. It was so graded that the thickest part of the one size corresponded to the thinnest of the following size. But the instrument, though important, was not everything. The operator counted for a good deal; and a faulty instrument would be efficient in the hands of a dexterous operator.

*Dr. A. Wallace* said that the instrument had the disadvantage that it involved the necessity of working with a straight dilator in a curved canal; and it would be rather a difficult matter for the operator to be sure where he was going. *Dr. Sloan's* instrument followed the natural curve of the pelvis. The results of *Dr. Mackintosh*, in 1834, in which, by the old fashioned methods he cured 28 out of 34 cases of sterility, would probably not be much improved upon. He used steel bougies, and with good results both as regarded sterility and dysmenorrhœa.

*Dr. Oliphant* said that within the uterus, the axis was quite straight; the curve was outside the uterus.

*Dr. Moffat* enquired whether the speculum was used by *Dr. Reid* in connection with his instrument?

*Dr. Stirton* said that it appeared to him that the power which the operator could exercise by means of the screw was very great; and that also he could not know the amount of force he was exerting on the uterus. Many years ago, he was in company with the late *Sir James Simpson* when a *Dr. Ritzius* was showing him a screw dilator of the same kind, but with the point a good deal blunter, and with another contrivance which gave it the advantage of *Dr. Reid's* instrument—viz., a bend corresponding to a sound.



*Dr. Simpson* said that he would try the instrument, but he said that its power was something horrible. It was impossible to have hæmorrhage in any quantity with such a force as that,—a force under which the uterus would inevitably contract. His objections were no doubt purely theoretical and speculative, as he had never tried the instrument.

*Dr. Reid*, in reply, said that the cases in which he stated he had failed were cases of dysmenorrhœa, not of sterility, to only one case of which he had adverted as a cure. In the cases of failure, he had simply erred in diagnosis, the event proving that they were not due to a stenotic condition of the os internum, but to some other cause, for which dilatation was inapplicable. The hæmorrhage was not more than a teaspoonful; bleeding in any large quantity in these cases was not anticipated, though in dilatation, under the old system, he had occasionally seen considerable bleeding. In regard to how he knew that the point of the instrument was going in the right direction, he must explain that, in applying it, he was not pushing on the point, but what happened was this, that the uterus really came down over the instrument. Unless it were pushed to the fundus he did not well see how it could damage the uterine walls. There was no doubt that the instrument passed up with considerable force, but with nothing of the force with which it would be propelled by an ordinary male screw acting in a soft female one. He was really not aware that he had been anticipated in working out the idea of this instrument; but there was nothing new under the sun. The dilator was always used in connection with the speculum.

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## PATHOLOGICAL AND CLINICAL SOCIETY.

SESSION 1886-87.

MEETING IV.—13TH DECEMBER, 1886.

*The President* DR. JAMES FINLAYSON, *in the Chair*.

I.—DR. JOSEPH COATS showed a specimen of SACRAL TERATOMA which had been sent to him by Dr. M'Farlane of London Road.

II. DR. DAVID NEWMAN showed a specimen of BOWEL FROM A SEVERE CASE OF OBSTRUCTION. The whole of the colon was

enormously distended, and at one point there was effusion of blood beneath the mucous membrane, above which a minute puncture had occurred. There was no fibrous band and there was no recent intussusception, but there may have been an old one, which had set up inflammatory changes.

### III.—PAINFUL SUBCUTANEOUS GROWTHS IN A PATIENT WITH A RHEUMATIC HISTORY.

By GEORGE S. MIDDLETON, M.D.

A woman, aged 39, suffering from numerous painful, subcutaneous growths on the fingers of both hands, and with a rheumatic history of more than twenty years' duration was shown. The tumours, mainly on the flexor aspects, were mostly adherent to the skin; but tumours were also observed in the sheaths of the tendons on the knuckles. The case came under his notice at the Royal Infirmary Dispensary, and was quite unique in his experience. It appeared also to differ materially from those described by Drs. Barlow and Warner at the London Medical Congress in 1881.

*Dr. Gairdner* was satisfied that this case differed from cases described by Drs. Barlow and Warner, and if *Dr. Middleton* would search through the volumes of *Clinical Society's Transactions* he would find allusions to similar cases.

*Dr. Mucewen* would be glad to make a cast of the case, and to remove some of the growths for examination.

### IV.—CASE OF BLINDNESS FOLLOWING AN ATTACK OF RIGHT HEMIPLEGIA.

By CHARLES WORKMAN, M.D.

Robert M'C., æt. 46, engineer, admitted 11th November, 1886, complaining of almost complete loss of sight of both eyes.

*Previous History.*—Patient has always been a fairly healthy man. Twelve years ago he was laid up for a week with sun-stroke. Sixteen years ago he had a bubo, and nine years ago had gonorrhœa. He admits no syphilitic history. His father died at 65 of cancer. Mother still lives. One brother and two sisters died of phthisis, another brother died young, and a sister suddenly from some head affection. Has three brothers and three sisters alive and healthy.

*History of Present Illness.*—Seventeen months before admission he had an attack of right hemiplegia, the right side of his face was paralysed, and his speech was affected.

At the end of a week he was able to be up and go about a

little, but has not yet completely regained the power of his right arm. Ever since the shock he has been troubled with dimness of the right eye, but up till four weeks before admission he was able to read the newspapers with his glasses; the blindness then rapidly increased, so that by the 26th of October he was only able to distinguish light from darkness.

*State on Admission.*—The only remains of the hemiplegia appeared to be some slight weakness of right arm and hand. On examining the chest the only alteration of note to be found was a v.s. murmur heard best over the base of the heart, and at the second right costal cartilage. Except for this and the blindness the man appears fairly healthy. The eyes on examination show—Right pupil dilated; does not react to light. Left pupil not so widely dilated, and reacts to light. With right eye he appeared quite unable to distinguish light from darkness, but with the left he could perceive the shadow when anything passed between him and the light. With the ophthalmoscope there was found some hyperæmia of both discs, the inner edge of the right disc being especially blurred with exudation. To the outer side of left disc the sclerotic ring was very strongly marked as a white edging. The rest of the fundus appeared healthy. There was gradual improvement in sight till the 23rd November, the patient being able to tell how many fingers were held up and to see the bars of the windows opposite.

On the 24th I found him suffering from great pain in left eye and side of the head. The vessels of the sclerotic were much congested, and sight appeared to be quite lost in that eye. The tension appeared to me to be decidedly increased. Dr. Wood Smith ordered leeches to the temple, and eserine drops to be put in the eye. In a few days this glaucomatous attack passed away, and the sight has been improving in both eyes.

In this case we have blindness occurring without any intra-ocular lesion sufficient to account for it, but occurring in a man who has valvular disease of the heart, and who has suffered from hemiplegia, most likely due to embolism.

I incline to think that in this case the loss of sight is due to interference with the circulation in the corpora-quadrigenina, or perhaps in the optic thalami by embolism. The patient may have been at first more blind than he thought, with the right eye, and then a second lesion may have affected the left.

V. DR. DAVID NEWMAN showed a CASE OF MALFORMATION OF THE FACE.

## VI.—CYSTIC CARCINOMA OF THE MAMMA.

BY MR. H. E. CLARK.

The specimen shown was a CYSTIC CARCINOMA OF THE BREAST, removed from a woman aged 54. The tumour was of two years' growth, but had grown very slowly till about six months before admission into hospital. The skin was adherent to the tumour at one point, and here the veins were much distended and varicose. The base of the tumour was hard and had the dense and heavy feel so common in scirrhus of the breast, but at the adherent part it was soft and fluctuant. It was supposed that this fluctuation was due to suppuration, but after the tumour was removed the fluid part was found to be cystic, being formed mainly of one cyst of considerable size, but with one or two smaller cysts at its base. The glands in the axilla were unaffected.

Dr. Newman examined the tumour and gave the following report:—The tumour consists of dense fibrous bands, radiating from the region of a cyst situated just under the skin a short distance from the nipple, which is markedly retracted. The cyst presents smooth walls somewhat sacculated from elevated bands of dense firm tissue similar in appearance to the fibrous bands throughout the tumour. The skin over the cyst wall is intimately adherent.

Microscopically the dense fibrous looking tissue is found to consist of fully formed fibrous tissue, with few nuclei, enclosing irregular meshes filled with epithelial cells, and in the large spaces a large quantity of granular material, probably derived from breaking down of epithelial cells.

The epithelial cells generally appear to be in a state of granular degeneration. The tumour presents the characters of carcinoma of a very slow-growing type, and probably is a specimen of that variety of cancer designated adenoid carcinoma.

*Dr. Dalziel* showed microscopic sections and thought that case was one of adeno-carcinoma. In the interior of each loculus was a granular mass.

*Mr. Maylard* thought Mr. Clark's designation of cystic carcinoma was more correct than that of adeno-carcinoma, the latter term applying more to a tumour beginning as an adenoma.

*Dr. Newman* would call it an adeno-carcinoma, and thought both descriptions were consistent with the appearances presented.

*Dr. Coats* had a difficulty with the name of adeno-carcinoma.

*Dr. Macewen* had shown at the discussion on cancer a specimen of adeno-carcinoma, and on another occasion he had presented a case where there was a cyst in the tumour.

*Dr. Dalziel* explained that *Ziegler* used the classification which he had employed in connection with the specimens shown at the the meeting.

VII.—PERFORATION OF THE ŒSOPHAGUS AND PENETRATION OF AORTA BY A FISH BONE; FATAL HÆMORRHAGE.

BY PROFESSOR GAIRDNER, M.D. AND JOSEPH COATS, M.D.

The specimen showed the œsophagus and aorta laid open, and exhibited the following conditions:—In the œsophagus, at a point about  $2\frac{1}{2}$  inches below the level of the bifurcation of the trachea, are two oval apertures—one on either side—that on the right side being rather higher than that on the left. The former measures half an inch in long diameter, and the latter three-quarters. Each of them presents an absolute gap in the whole wall of the œsophagus, and in both of them, at the upper and lower extremities, the mucous membrane is divided somewhat farther than the other coats, and the division tapers off to a sharp point, especially at the upper end. It is as if the lesion had been made by a sharp knife, dividing the coats from within. The aperture on the right side communicates with a large cavity which passes towards the root of the lung, extending chiefly downwards outside the œsophagus, and lying to some extent between the latter and the pericardium. That on the left side also communicates with a large irregular cavity, which passes, however, more upwards than downwards, and lies chiefly between œsophagus and aorta. This cavity contained blood and shreddy decomposing matter. In the aorta a very ragged irregular aperture is visible, measuring three-eighths of an inch in long diameter, which is obliquely from above downwards, and a quarter of an inch in transverse diameter. The internal coat has been greatly displaced from the surface of the aperture, and floats about still attached to the edge and partly replaceable. There is one place where the internal coat is attached at both edges, forming a bridge across the aperture. The aperture in the aorta is slightly below the level of that on the left side of the œsophagus, but directly in a line continued from the middle of the aperture on the right through the middle of that on the left side.

The stomach was found very greatly distended, chiefly

with blood, to a large extent coagulated, which was found to weigh 4½ lbs. The intestine also contained blood whose distribution was in general as follows:—The duodenum contained a considerable quantity. There was very little in the jejunum, and it became again abundant in the ileum, but it was most abundant of all in the large intestine, which was considerably distended with it, the blood here being of a tarry consistence. It extended right down to the anus, and some was found projecting externally. This distribution of blood may possibly mean that there were two distinct hæmorrhages, or else that the blood which passed beyond the stomach was hurried through the small intestine, and again accumulated in the large. It is well known that this is the case in general with the contents of the alimentary canal.

The history of the case having pointed to a fish-bone as the cause of the perforations, diligent search was made in the contents of the stomach and intestine, in the cavities communicating with the œsophagus, in the aorta, and in the arteries of the lower extremities, but without success.

*Clinical Statement.*—P. R., æt 63, labourer, was admitted to the Western Infirmary on 23rd November, 1886, with pain in the sternum and in the epigastrium; the action of the heart being irregular, with frequent intermissions. He had swallowed a haddock bone eight days before admission, while in a state of intoxication; and no information could be obtained as to whether it had been dislodged or not. For some hours after admission there was little or no complaint of the pain, even during his meals; but shortly after midnight, on the 24th, he had a fainting fit, during which he fell on the floor, and had to be assisted into bed; great pain in the chest following, and being only subdued by an opiate. At 11 A.M. there was a somewhat similar attack, but less severe; on this occasion he had a desire to go to stool, but fainted before he could pass anything, and was put back into bed. At 3.40 P.M. of the same day he had another attack of the pain, and this time he vomited up two or three mouthfuls of red blood, and died somewhat suddenly thereafter, about 4.20 P.M. His bowels had been moved the day previous to admission, but not while under observation. His urine contained a trace of albumen. As he was without near relatives no further particulars could be obtained; but it was known that he had been invalided from the Army in or about 1850, on account of a "weakness about the heart."

*Dr. Gairdner* said that in this case the clinical history pointed with great probability to two or three distinct hæmor-

rhages—perhaps more—any one of which might have proved fatal. The pathological facts corresponded, so far, with this, for blood was found in great abundance in the stomach (of course arising from the last, or fatal, hæmorrhage), and again in the colon, while the tract of the small intestines was for the most part quite devoid of bloody, or even of blood-stained, contents. The important practical conclusion is, that if a man can live for some hours after the aorta has been penetrated from without by a foreign body, there are, or may be, possibly favourable conditions under which such a breach might be permanently healed. Thus, pathological anatomy, often so stern in its teachings as regards the fatal issues of disease and injury, may sometimes, on more careful consideration, be made to undo the extreme gravity of its own lessons. But, unhappily, the most of what he had personally learned from pathological anatomy at an earlier date as regards fish bones and their results, had been of a kind to inspire very great distrust. A few such incidents might be mentioned, although some of them were already in print, in a more or less complete form of narrative for reference. In Dr. Gairdner's *Clinical Medicine*, at p. 14, will be found details of the case of a middle-aged woman who, though she was known to have swallowed a fish bone some days before, was supposed by herself and others to have got rid of it, through surgery. The acute local pain had disappeared, but she was believed to have caught fever, and found her way accordingly into a fever ward in the Royal Infirmary of Edinburgh. It was certainly not a case of contagious fever, but rather seemed to be one of double, and therefore probably diathetic or septic pleuro-pneumonia, attended by extreme depression, almost like the collapse of cholera. She died within forty-eight hours after admission. The *post-mortem* examination showed that quite a series of evidently pyæmic inflammations, including both pleuræ, the lungs, and the pericardium, had sprung from this foreign body, which had not been extracted as supposed, but very probably may have rather been impacted in its ultimate situation by surgical interference, having been found quite surrounded by pus in the cellular tissue behind the lowest part of the pharynx. In remarking on this case (which was mentioned in a discussion in the Medico-Chirurgical Society of Edinburgh, 5th January, 1859, the late Professor Spence and others made some valuable remarks upon the surgery of such cases, showing that there was danger in too much, as well as in too little, interference. (See *Edin. Med. Journal*, vol. iv, 1859, p. 770). Another case

was not less instructive, for it showed that a fish bone might pass into the small intestines without doing much injury by the way; and then might penetrate into the peritoneum with the effect of setting up fatal, although chronic, peritonitis, at an indefinitely remote date afterwards, when every trace of the history of the accident had been lost. In this instance the fish bone was found matted by adhesions between two coils of small intestine near the middle of the abdominal cavity, with several pints of serum effused, and all the other phenomena of peritonitis which, though not tubercular, resembled tubercular peritonitis in other respects. The patient, a superintendent of police, was supposed to have had disease of the kidney, brought on by over-anxiety. The third case mentioned in this discussion was the most curious of the three, and must have entirely baffled clinical diagnosis, even under the most favourable circumstances. A man was received into hospital suffering from cerebral symptoms; he died quite suddenly shortly afterwards. Several large abscesses, lined by pyogenic membrane, were found in the substance of the brain, the meninges being comparatively free from disease. The remote cause of these abscesses remained obscure until, on further examination, a fish bone, imbedded in pus, was found immediately beneath the serous membrane upon the lower surface of the liver. Of course, it was at once inferred that this foreign body had at one time or other been in the intestinal canal, but the course of its transit from this to its ultimate and very unusual position was by no means apparent; there were no general adhesions between the liver and the adjoining viscera. At last, and only after a close scrutiny, a minute cicatrix was found in the duodenum, perfectly healed, but still probably indicating the seat of a former perforation. The moral of these latter cases unhappily was, that fish bones were at least possible sources of danger, long after the primary and recognised symptoms connected with their passage through the intestinal canal may have been lost sight of; and even the original cause of the disease existing may have been forgotten, just as, in a case related to the Pathological and Clinical Society on a former occasion, it had been shown, on *post-mortem* examination, that a chronic disease of the lung, attended by excavation and gangrenous changes, had sprung from a fragment of a metallic pen passing down into a bronchus, probably in infancy, and unknown to the mother of the child, or forgotten. On the other hand, many facts are on record—some of them



even marvellous, showing the comparatively safe transit of needles, pins, &c., and even much larger and more complex foreign bodies through the organism, to points very remote from the seat of their introduction into it.

*Dr. Finlayson* referred to a case of perforation of the œsophagus which had been under his care.

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## GLASGOW SOUTHERN MEDICAL SOCIETY.

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SESSION 1886-87.

MEETING VII.—27TH JANUARY, 1887.

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MR. EDWARD MACMILLAN, *Vice-President, in the Chair.*

### I.—CASE OF HEARING-MUTISM.

A discussion took place regarding a case simulating hearing-mutism which had been shown by Mr. Stuart Nairne at the previous meeting of the Society. He considered the case as one of glosso-labio laryngeal and pharyngeal paralysis.

The patient, a boy, 13 years of age, had scarlet fever and convulsions when 16 months old, and has suffered from chorea ever since, which is sometimes better and sometimes worse. Spasmodic movements of the arms, head, and legs are noticed, and the boy walks fairly well, but with an irregular movement. His tongue is gathered up in his mouth, and appears somewhat more posterior than natural, and also of unusual length. The hearing is perfect. The patient cannot read, but he can sing. He can repeat all the vowels in a spasmodic way. With regard to the consonants, he pronounces *m* very well, and also the gutturals, but the others imperfectly or not at all.

*Dr. James Erskine* could not agree with Mr. Stuart Nairne in his diagnosis of the case under discussion; but assuming that it was a case of bulbar paralysis, it was not remarkable to find it associated with good hearing, which was generally observed in that disease. That fact, however, was notable on account of the fibres from the auditory nuclei in the pons and floor of the fourth ventricle, lying in proximity in the medulla oblongata to the nuclei of the various nerves affected in bulbar paralysis. He thought that the case which had been brought

before the Society was not one of bulbar paralysis, in view of the very distinct history and symptoms of chorea which existed. It would be interesting to know how much of the defect of speech observed in the patient was owing to that disease, and how much, if any, was the result of paralysis. The patient seemed imbecile in some degree, which might account for his defective utterance of articulate sounds. Dr. Erskine denied that the boy was a mute, because he still had a limited power of phonation and articulation. Mutism was not a defect, but the absence of speech. A hearing mute was a very rare phenomenon as compared with a deaf mute. In the latter case the organs of speech were perfect and the person suffering from this infirmity who was only deaf could be instructed to speak. After some remarks on the oral method of teaching the deaf and dumb,

*Dr. Glaister* made some remarks upon the case under discussion. He had known the boy for several years, and having had the opportunity of examining him carefully, he had come to the conclusion that he was not suffering from bulbar paralysis. That disease, he pointed out, was peculiar to adult life. There was a history in the present case of scarlet fever and convulsions, followed by chorea, from which the patient still suffered. He granted there was paresis to some extent, as shown by the imperfect action of certain muscles; for instance, he could not whistle, nor could he blow out a match if his nostrils were closed. Regarding the intelligence of the boy, he was at first inclined to think he was imbecile, but he had altered his opinion. He might appear imbecile on account of the choreic movements. The parents thought the boy was imbecile till lately. The backwardness in intelligence might be due in some measure to brain lesion, or to want of education, or to both. The boy could not sing, but he knew the difference between several airs. He evidently has no notion of musical tone, but can make a difference in tone. Dr. Glaister did not think the case was one of bulbar paralysis. He regarded it as chorea, and noted that paresis was present along with inco-ordination of muscular action.

*Dr. Workman* also thought the case was not bulbar paralysis, and

*Dr. John Brown* regarded it as chorea affecting muscles, unusually involved in that disease.

*Mr. Nairne*, in reply, stated that he was pleased that the criticism of his remarks had been so freely given, as he had just brought forward the case for the purpose of obtaining information regarding it. As he considered that there still

remained some points in connection with it, he proposed that a committee of the Society should be appointed to examine the case and report.

The chief points in the committee's report are:—A lesion of the brain might have taken place at the time the boy suffered from convulsions, but that no evidence of that had been noticed, so far as speech was concerned, till he became of a speaking age. In addition to that, the chorea had given the impression that the boy was imbecile, which has only recently been found not to be the case. The chorea has affected the muscles of face and neck, tongue, palate, and the muscles concerned in vocalisation. It also affects all the muscles of prehension and locomotion, and this is observed more on the right than the left side of the body. The arch of the roof of the mouth is somewhat exaggerated.

The committee recommended that the boy should be taught as an ordinary deaf mute by the digital and sign method and not orally, as, on account of the slight faculty of phonation and articulation he possessed, he could not acquire the latter method of communication.

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#### MEETING IX.—24TH FEBRUARY, 1887.

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MR. EDWARD MACMILLAN, *Vice-President, in the Chair.*

##### I.—NOTES OF A CASE OF PERICARDIAL ADHESION.

BY DR. JAMES HAMILTON.

The heart was shown. The patient, a girl of 6½ years, had not suffered from acute rheumatism, but her illness was of a rheumatic character. The temperature had risen to 103°, and sweating had been profuse during the time she was under observation, ten months before the fatal illness. A systolic murmur at the apex of the heart was then detected, soft, and blowing in character, and the apex beat was slightly diffused. Neither friction nor enlarged cardiac dulness was discovered. The cardiac condition had attracted no attention on the part of the parents, and Dr. Hamilton had been called in on account of an inflamed gland on the right side of the neck. Under appropriate treatment the inflammation subsided, but the patient was not seen again till a few weeks before death, although her condition was fully explained.

About eight weeks before death the child had taken hooping-cough, but this had continued for six weeks previous to her being again brought under observation. It was then found that the cardiac symptoms were much more pronounced. The murmur, which was still only systolic, was heard all over the chest, but most loudly at the apex, and there was a distinct purring tremor communicated to the hand. The cardiac dulness was also much enlarged. Capillary bronchitis had set in along with œdema of the feet and legs, and general anasarca, and the patient died from dyspnoea. On opening the chest eight hours after death the thorax was found full of fluid, and the heart was seen to occupy the whole of the left side in front, extending beyond the middle line, the left lung being quite hidden from view. Extensive pleuritic adhesions were present, especially on the right side, where the pleura was also adherent to the pericardium. The heart, on being detached from its connections, was found to be closely attached to the pericardium; the pericardial sac being entirely obliterated. It was greatly hypertrophied, and weighed thirteen ounces. All the valves appeared healthy, with the exception of the mitral, on which a number of small vegetations were present.

*Dr. Lindsay Steven* said he had examined sections of the heart and pericardium under the microscope, and had found the appearances observed in ordinary rheumatic pericarditis, in which the fibrine had not been absorbed, and was still remaining between the adherent layers.

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#### MEETING X.—10TH MARCH, 1887.

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MR. EDWARD MACMILLAN, *Vice-President, in the Chair.*

#### I.—CLINICAL MEMORANDA.

BY DR. J. LINDSAY STEVEN (p. 428, vol. xxvii).

Dr. J. Lindsay Steven read clinical notes of cases of peripheral paralysis, the previous part of his paper, consisting of clinical notes of cases of paraplegia, having been read at the meeting of the Society on 24th February.

*Dr. Couper* described two cases of locomotor ataxia which occurred after exposure to cold and wet. One case was treated by the late Dr. Scott Orr with croton oil pills, and recovery

took place. The other case was treated successfully in the same way. He would like to know whether Dr. Steven considered the treatment by ergot preferable to croton oil. Dr. Couper thought it was very satisfactory to note that serious lesions of the spinal cord can be treated successfully.

*Dr. Glaister* had treated chronic paralysis successfully by means of the application of the actual cautery, which he had found very useful in the treatment of such cases. More attention was being devoted to the subject of peripheral paralysis. In one case of that affection he had a good result from treatment by embrocation with oil and turpentine in addition to massage.

*Dr. Barras* was of opinion that the suppurating finger mentioned in one of Dr. Steven's cases might have resulted from atrophic changes. He described a case of paraplegia (not myelitis) from alcoholism which ended in dementia. The paraplegia was not complete, but the patient could not stand.

*Dr. John Brown* read notes of a case of anæsthesia without paralysis as bearing upon the subject under discussion.

*Mr. Macmillan* thought the actual cautery was not often enough used at the present day, and was of opinion that some benefit could be obtained by its means in such cases as those described by Dr. Steven. He also approved of the old treatment of purgation by croton oil.

*Dr. Lindsay Steven* replied, and remarked that by depleting the spinal cord croton oil would have acted fairly well, but in one of his cases he would have been afraid to use it from fear of exhausting the patient. Ergot acts by contracting the dilated vessels in the cord, and diminishing exudation. By tiding over the wearing-out period in such diseases as those under consideration, and by keeping up the strength of the patient and preventing the destruction of the elements in the cord, a recovery would likely take place. Dr. Steven recommended local blood letting, and suggested that the patient should be made to lie with his face downwards. He concluded his remarks by commenting upon the widespread anæsthesia present in the case reported by Dr. Brown.

GLASGOW OBSTETRICAL AND GYNÆCOLOGICAL  
SOCIETY.

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SESSION 1886-87.

MEETING VI.—23RD MARCH, 1887.

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DR. SLOAN, *President, in the Chair.*

I.—COCAINE AND BORIC ACID.

By DR. SMITH, of Motherwell.

Cones of cocaine and boric acid, as recommended by Dr. E. Hend Moore for use in primiparæ, and cases of rigid os, were shown.

II.—WIRE CLAMP.

By MR. J. STUART NAIRNE.

Two simple forms of wire clamp were shown, and Dr. W. L. Reid passed a favourable criticism upon them.

III.—AMPUTATED CERVIX.

By DR. MURDOCH CAMERON.

A cervix amputated for elongation by Prof. Leishman was exhibited. The patient was a nullipara. He advocated the use of the galvanic *écraseur* for such cases.

IV.—SPLEEN AND UTERUS.

By DR. SLOAN.

The spleen and uterus with appendages of a woman were shown, who died in the Maternity Hospital on the 22nd day of the puerperium. His house surgeon, Dr. Dumoulin, read the notes of the case from the journal, and showed the temperature chart. From these it appeared that M. W., æt. 22, a primipara, entered the Maternity on 8th February, 1887. She was unmarried, thin and delicate looking. For two months previously she had had no home, and had slept in closes or walked the streets frequently all night. Sometimes she had passed as long as 48 hours without food. She had been walking about all night previous to her admission, and had had no food for 24 hours. On the 13th, at 11.44 a.m., she was delivered of a mature female infant, weighing 6½ lbs., the presentation having been the third cranial, and the duration of labour 36 hours for the first,

4 for the second, and 15 minutes for the third stage. Aid was given during the second stage by means of Simpson's axis traction forceps; and these being removed, the third stage was precipitate, a rupture of the perineum being the result. This was stitched immediately, and the vagina douched. The stitches were removed on the third day for tension.

On admission, her urine was found to be albuminous to the extent of a third, and it continued in a similar condition after labour.

Her temperature on the 13th was  $101^{\circ}$ , and it continued pyrexial throughout, rising to  $104.4^{\circ}$  on the 21st,  $105.2^{\circ}$  on the 23rd, and to  $105.8^{\circ}$  a day or so prior to death.

Soon after labour she became very thirsty, and had frequent perspirations. On the 15th diarrhoea (7 motions) was noted; she passed several clots and had a rigor. On the 18th considerable uterine pain and hysterical phenomena are recorded. From the 19th to the 24th she *felt* fairly well, though on the 21st she had a rigor. On the 25th she had a numbness in her right leg from the knee to the toes. On the 26th pain was experienced in the right popliteal space. On 2nd March she had convulsive movements of the right arm, and on the 4th painful swelling of the left wrist. On the 5th quivering of the legs and arms was a prominent symptom, and on the 6th sordes formed on the teeth and lips, and rapid respiration (72 per minute) preceded dissolution at 7 p.m.

At the *post-mortem* examination the kidneys were found macroscopically normal, and involution of the womb was well advanced. The spleen was in process of healing after rupture.

The day before death the perineal wound looked very unhealthy.

The *President* then remarked that there was apparently a small tear of the cervix which had completely healed. The thickening at the placental site might be supposed by some to be a portion of placenta; but he could assure them that this prominence was scarcely, if at all, visible at the *post-mortem*, and it had probably arisen from the action of the spirit. The whole of the interior, indeed, was noticeable as being specially fresh and healthy looking. Besides, the advance of involution was evidence of the absence throughout of any putridity there. The spleen, which was slightly increased in size, had shrunk from the action of the spirit, and there was now no indication of the cavity mentioned in the report. He had been unable to find a similar condition of spleen reported; though the late Sir James Simpson knew of three cases of fatal rupture of this organ. Two of these cases had proved fatal shortly after labour; the remaining

one a week or two after delivery. In these cases, however, the coats of the spleen had ruptured; whereas in this case the coats had remained entire. He believed that this internal laceration had been caused by the severe strain of the obstructed labour, and that it might account for the prostration of the woman at the close of labour. The section of the spleen at the seat of injury which was under the microscope, they would notice was indicative of a process of repair—of a change into fibrous tissue—and had no appearance of having been at any time in a purulent condition.

He would like to try to answer the question—What did this woman die from? It was evident that she nearly died from the effects of the labour, and was in danger of succumbing to the ordinary perils of the puerperium. The diarrhoea had also a share in causing death; and it was a question whether this condition was a result of the starvation and exposure. It must be admitted, however, that some putrid poison had entered the blood, absorbed from the extensive perineal laceration, which had early become inflamed, and had had for a considerable time a diphtheritic appearance, though there was certainly nothing of the nature of a diphtheritic membrane on the unbroken surface of the vulva or vagina. Did she then die from septicæmia, or did she die with sapræmia, but also, if not mainly, from other causes? He explained the latter term, which he believed had been first used by Dr. Matthews Duncan, to mean blood with putrefying matter in it—the former being blood with putrefied matter in it. In a case of sapremia, if only the further supply of the poison were prevented, as by syringing a uterus in which putrid lochia lay, in a brief space of time the poison was eliminated from the blood, and the fever subsided; either because the poison was not of the nature of a ferment or because the blood had proved an unsuitable soil for its development. Now, in the case of this girl, the blood was surely in the most suitable condition for the development of any poison capable of developing; yet, notwithstanding the fact that the patient lived three weeks after delivery, there were no signs of septicæmic inflammation, nor of purulent deposition. The clots found *post-mortem* in the heart and axillary vein were aseptic, and had evidently formed during the slowing of the blood current immediately before death. Though there had been some phlebitis of the leg, and some pain and swelling of the wrist, the former had disappeared with the simplest of treatment; whilst at the *post-mortem* there was nothing to indicate that the latter had ever existed. Was the seat of origin of the poison the



cause of its comparative innocence? If the cervix, instead of the perinæum, had been seriously bruised and torn, or a clot or a piece of placenta been left in the uterus, would there not have been an acute septicæmia with rapid death, or a less acute form ending in multiple abscess? Another point he suggested for discussion was, Did the fact of the patient getting out of bed (which he always permitted in cases of mental distress where practicable) break up the base of the perineal ulcer, and permit an easier entrance of the poison into the blood? Regarding the hypodermic injections of eucalyptus oil, he stated that this was only the second case in which he had given this treatment a fair trial, and though many large abscesses had been in existence before the treatment was begun in the first case, and the patient was to all appearance moribund, she nevertheless recovered.

*Mr. Nairne* remarked upon the anomalous fact of highly albuminous urine, and the apparently healthy condition of kidneys as shown *post-mortem*.

*Dr. Murdoch Cameron* inclined to the view that the case was one of septicæmia.

*Dr. W. L. Reid* thought more stress was to be laid on the condition of the recipient blood than on the nature of the poison itself.

*Dr. Pollok* thought it possible an abscess might have formed in the spleen and been absorbed, causing pyæmia.

*Dr. Oliphant* raised the question of fatty embolism in the lungs, induced by the eucalyptus oil injections hypodermically.

The *President*, in reply to *Dr. Reid*, said he felt that the absence of pathological evidence of septicæmia, together with the fact that there had been plenty of time for its development, was against the view that death was due to that condition. He thought *Dr. Oliphant* had failed to notice that lividity was reported to have been a marked symptom before the hypodermic administration of eucalyptus oil was resorted to, and that the dyspnœa was certainly due to bronchial obstruction. He could not see that the oil could do any harm to the lungs; it had not done so in the other case in which it had been used. As to other cases in which this treatment had been tried for a few hours before death, he pointed out that he did not say that he had only tried it twice, but that he had only given it a *fair* trial in these two cases. He explained, in reply to *Dr. Park*, that it was the first quality of the ordinary oil, and not *Saunders'* eucalyptus extract, which had been used, and that it was mixed with the finest olive oil in the proportion of 1 to 4, and of this 20 minims were injected at each dose.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

### SURGERY.

By MR. A. E. MAYLARD.

**On the Diagnostic Value of Examination per Rectum in Coxitis.** By Dr. Arnold Schmitz, St. Petersburg.—“The determination of the question, in what part of the structures forming a joint a morbid process, whether simply inflammatory or manifestly tuberculous, first began, is undoubtedly of inestimable value as bearing on treatment. It appears to me that a method of examination in disease of the hip-joint, which may often give most important information, has not the attention paid to it that it merits, and the aim of this communication is to bring it now into notice. I refer to palpation of the posterior (internal) surface of the acetabulum through the rectum. In illustration of this I venture to describe shortly the three following cases:—

“I. A boy, aged 3 years, was stated to have been limping and also screaming out during his sleep for the last three months. Persistent malposition of the left thigh (marked flexion, with slight adduction and rotation inwards), diminished movement in the hip-joint, and a large abscess over the trochanter, pointed clearly to coxitis. There was, however, some doubt occasioned by the fact that under chloroform the joint could be moved freely in every direction except that of extreme extension without detection of crepitation, and besides a slight prominence of the third lumbar spinous process, suggested spondylitis and abscess gravitating through the great sciatic foramen, or into Petit's triangle. Here exploration per rectum was at once decisive, for it revealed the presence, on the inner surface of the pelvis, opposite the position of the acetabulum, of a distinct, circumscribed swelling, not, however, fluctuant. The case was thus plainly one of acetabular coxitis. The gluteal abscess was freely opened, and a communication between it and the hip-joint discovered. After opening the capsule of the joint sufficiently and removing the cartilage, apparently in itself sound, but not firmly attached to the bone, from the head of the femur, it was seen that the whole of the cavity of the acetabulum was diseased and perforated by caries, so that after removal of two sequestra, each the size of a bean, two fingers could be pushed through into the pelvis. All the carious bone and the diseased soft parts were removed.

“II. A very pale boy, aged 5 years. Chronic nephritis after scarlet fever (a year and a half before). Both thighs acutely flexed, the right from habitual position during long confinement to bed, the left as the result of inflammation of the hip-joint. A large collection of pus under the gluteus upon the left side. By examination per rectum there could be felt an abscess the size of a hen's egg behind the acetabulum. This explained the statement of his father that for a considerable time there had been severe pain during defecation. Under chloroform, no roughness in the joint could be detected. The still intact head of the femur was resected, and the fungous capsule removed. A sequestrum the size of a bean was removed from the floor of the acetabulum, which gave exit to a large quantity of pus from within the pelvis. The whole of the floor of the acetabulum was removed by the chisel.

“III. A boy, 5 years old, affected with very marked thoracic and lumbar spondylitis, had limped since August, 1885. The left thigh was immovable, fully flexed, and slightly adducted over the trochanter; deep fluctuation in a limited area. An abscess, the size of a walnut, was detected behind the acetabulum by exploration per rectum. Resection of the head of the femur, with healthy looking cartilage. Synovial membrane fungous. Perforation

of the acetabulum admitted the finger freely into the cavity of the pelvis, and gave vent to a considerable quantity of pus.

"In the second case, the diagnosis of disease of the hip-joint was clear, even without the rectal examination, but in the two others, especially in the first, it was only by that examination that the diagnosis was fully cleared up. At all events, in all three the rectal palpation confirmed the opinion that there was primary acetabular coxitis. The first case seemed especially instructive to me, where there was no abscess, but where the detection by the finger of the circumscribed doughy swelling behind the acetabulum showed in what position of the joint the disease originated. . . .

"It is as yet questionable whether, by means of rectal palpation, all cases of 'pelvic coxitis' ought not be diagnosed at a very early stage. The method is of great significance bearing on treatment. Early operative treatment, chiselling out the diseased acetabulum before the external manifestation of suppuration, or of recognisable grating in the joint upon movement, might be clearly indicated, thus obviating the risk of an attempt at spontaneous elimination of an acetabular sequestrum, causing total destruction of the head of the femur, or perhaps an abscess breaking through into the rectum. The experience noted above determines me in future never, in examining a case of an affection of the hip-joint, to omit rectal palpation."—(*Centrabl. f. Chir.*, 13th March, 1886.)—D. M'P.

## GYNÆCOLOGY AND OBSTETRICS.

By R. STEVENSON THOMSON, M.B., C.M., B.Sc.

**Hysterorrhaphy.**—At a recent meeting of the Obstetrical Society of Philadelphia, Dr. Harvard A. Kelly read a paper in which, under the name of hysterorrhaphy, he advocated an operation which he thought was to be preferred to shortening of the round ligaments in certain cases of retroversion or retroflexion of the uterus. The speaker stated that he first did this operation on the 25th of April, 1885, in the case of a patient who had been under his care for nearly three years, and previously under that of several other physicians. The uterus was acutely retroflexed, the body large and soft, and the fundus lying below the level of the cervix. Months of rest in bed, accompanied with careful local treatment, failed to overcome the flexion even temporarily. A year before, the right ovary had been removed by the vagina. The left ovary and oviduct were now removed, and, on raising the uterus, the operator felt a sharp band of cicatricial tissue half encircling the organ at the angle of flexion, which made it evident that any attempt to correct the condition from without would prove futile. Silk sutures were passed through the left horn of the uterus, and the body of the organ was slung from a point on the anterior abdominal wall about an inch and a half above the pubes, to the left of the incision. The suspensory sutures were passed between two ligatures encircling the horn at the base of the pedicle, to avoid the danger of their teasing out and of bleeding. The uterus, thus suspended, remained in place for a year, when it was again dragged down by a distended state of the right oviduct.

Dr. Kelly thinks it would be well in future operations to suspend the uterus by both cornua. While in most cases, he added, the trouble would be found to be of long standing, and involving the appendages in chronic disease incurable except by their removal, in some instances it would be well to try the effect on them of the drainage to be secured by raising the body of the uterus, and with it the appendages. The operation was to be urged in cases where the uterus had long been retroflexed and infiltrated, and was incapable of maintaining its proper attitude after the removal of the appendages. If there were adhesions he thought they should be carefully severed.—(*New York Med. Jour.*, 27th November, 1886.)

**Erysipelas and Puerperal Fever.**—Gusserow (*Archiv für Gyn.*, Bd. xxv) presents a paper on this subject based upon an analysis of fourteen cases. After referring to the fragmentary state of our knowledge concerning the relation between these affections, he reviews the work of Hugenberger, Fehleisen, and others, and then calls attention to the discovery of the coccus of erysipelas as indicating the specific nature of the disease. From the history of the fourteen cases in which the latter developed after delivery, Gusserow shows that evidences of puerperal sepsis were present before the erysipelatous eruption appeared, the advent of the latter complication causing no additional symptoms, except an increased elevation of the temperature or pneumonic trouble. In fatal cases the ordinary evidences of septicæmia were observed; but in no instance was there any indication that the septic poisoning was connected with phlegmonous erysipelas. Experimental inoculations were made with pure cultivations of erysipelas cocci, rabbits being the animals selected. In every instance where the fluid was introduced beneath the skin of the ear a genuine erysipelatous inflammation was produced, attended with well marked general symptoms. Numerous injections of fluid containing cocci were made into the peritoneal cavity, but these never gave rise to any septic trouble—conclusive evidence that there is no direct relation between erysipelas and puerperal fever. Can septic uterine discharges, on the other hand, give rise to erysipelas? No cases have been observed in practice, and the bacteria characteristic of various septic and pyæmic inflammations have not yet been sufficiently differentiated to allow of any positive deductions from experimental inoculations. Assuming that no inflammatory process, however closely it may resemble erysipelas, can properly be regarded as the latter affection, unless its peculiar cocci are found, and can be cultivated and successfully inoculated, it may be affirmed that erysipelas has never been produced by inoculations with any septic matter derived from a patient with puerperal fever.—(*The New York Medical Journal*, 29th January, 1887.)

**Permanent Dilatation of the Uterus.**—Vulliet (*Arch. de Tocologie*, 30th October, 1886) has submitted to the Paris Académie de Médecine a paper setting forth a new method of dilating the uterine canal for diagnostic and therapeutic purposes, which was thought sufficiently important to be referred to a committee composed of MM. Tillaux, Cusco, and Charpentier. The following is a brief abstract of their voluminous report:—The advantage alleged by Professor Vulliet for his peculiar method is the opportunity which it gives to the gynecologist to inspect the diseased endometrium, and to apply medicaments directly to it. It is maintained that the dilatation can be kept up indefinitely, without injury to the patient, no matter to what degree it may be carried. The *modus operandi* is briefly this:—The patient is placed in the knee-chest position, the os exposed by the aid of a Sim's speculum, and bits of cotton, saturated in an ethereal solution of iodoform (one to ten), are passed into the uterine cavity. These tampons, which vary in size from that of a pea to that of an almond, and have each a piece of thread attached, are introduced by means of a sound, until the cavity and canal have been filled up to the os externum. After forty-eight hours they are withdrawn, and a fresh lot is introduced, and this operation is repeated eight or ten times, until the cavity has become so dilated that it is visible throughout its entire length. Laminaria tents may be used to supplement the tampons. After a sufficient amount of dilatation has been obtained, an intra-uterine speculum, devised by M. Vulliet, enables the physician to explore the cavity, to make local applications, to remove intra-uterine growths, &c., with ease, and with the certainty that he is not working in the dark. Vulliet reports several cases in which chronic endometritis, cancer, and submucous fibroids, both polypoid and intra-mural, were successfully treated by this method; other gynecologists are equally pleased with the results which they have obtained. The committee, after carefully reviewing the clinical evidence, decided that dilatation, as effected in the manner above described, was valuable, not only because of the perfect view which was afforded of the interior of the uterus, but, above all,

by reason of the fact that this dilatation could safely be maintained for weeks, or even months, during which time the progress of the disease, and the effect of the treatment, were literally under the eye of the physician. So far from there being any reaction from the presence of the tampons, the patients were actually relieved by them; the uterus seemed to tolerate the foreign bodies perfectly. The explanation of this singular tolerance probably lies in the fact that the uterus, after trying during the first few days to expel the tampons (as shown by the colicky pains of which the patient complains early in the course of the treatment), gradually loses its tone and ceases to contract. The constant contact of the medicated cotton with the mucous surface secures thorough antiseptics, and provides against the usual risks attending intra-uterine manipulations. M. Vulliet's method opens up great possibilities in the management of a certain class of affections, which have hitherto been treated empirically, if not blindly, and may introduce a new era in uterine surgery. The hope may even be indulged, add the committee, that the direct treatment of cancer of the endometrium will so far control that disease, that cases, in which hysterectomy is necessary, will become less frequent. "If M. Vulliet's process is not suitable for all cases, it is at least harmless. On the other hand, all of the patients who have been treated according to this method, have certainly been benefited. . . . *Primo non nocere*—this is the first, the grand principle in medicine. M. Vulliet's method is innocuous. The scientific standing of its proposer is a guarantee of the accuracy of his observations."—(*New York Med. Jour.*, 25th December, 1886.)

**Local Treatment of Membranous Dysmenorrhœa.** (*Nouv. Arch. d'Obst. et de Gyn.*, Jan., 1887.)—Landowsky, treating of the above subject, says, that membranous dysmenorrhœa results from a diathetic vice, or from a cachectic condition, though it is also found among apparently healthy young women. In the treatment the general conditions must be taken into account, while the local intervention must not be lost sight of. Landowsky, after dilating the uterus, applies the galvanic cautery to its internal surface five or six days after menstruation. The cautery should be heated a dull red. Patient confined to bed for eight days.

**Catarrh of the Uterine Cervix: its Electro-Therapeutics.** By Touret. (*Nouv. Arch. d'Obst. et de Gyn.*, April, 1887.)—Uterine Faradisation, which has for its object the dissipation of engorgement by producing uterine contraction, should be applied in all cases of chronic metritis, of hypertrophy of the uterine glands, and of uterine congestion. The electrical action produces an interstitial massage, and restores to the womb the tonicity it had lost, and, in addition, it regulates the circulation in all the organs of the true pelvis. The operation is extremely simple:—The positive pole, covered with a piece of moistened chamois leather, is applied to the abdomen, the negative pole is introduced into the uterus. The number of interruptions should be varied according to the tension and the intensity of the current, which should be progressively increased according to the sensations experienced by the patient.

Generally this treatment is sufficient to improve endometritis in a way to simulate recovery, and it is in these somewhat resistant cases that the author follows the Faradisation treatment by the application of the galvano-cautery, which acts by cauterising the tissues with which it comes in contact, and as a consequence, destroys the morbid tissues, the cause of failure.

**Preparations of Ergot.** (*Centralbl. f. Gyn.*, No. 20, 1886.)—The author (Kobert) confirms the results of his earlier observations.

1. Ergotinic acid does not cause any excitation of the muscular tissue of the gravid uterus.
2. Ergotine has not much action on the uterus when it is employed pure and crystallised.
3. Cornutine, on the other hand, excites the contractions of the uterus, and

acts on only one organ, the womb. Labour is the more easily excited in proportion as the pregnancy is nearer term.

4. Sphacelinic acid produces tetanus of the muscular tissue of the uterus.

Ergot kept for twelve months loses its activity completely. The author thinks it would be rational to discard ergot and ergotine, and only to employ cornutine.

## INSANITY.

BY DR. R. S. STEWART.

**Note on a Case of Sleep of Three Months' Duration.** By MM. Camuset and Planés (*Annales Med.-Psych.*, January, 1886).—A man, aged 36, of unknown antecedents, was admitted to the Vaucluse Asylum in May, 1883. His mental condition was one of subacute excitement, with irritability, vague ideas of persecution and grandeur, hallucinations of hearing, and troubles of general sensibility. He suffered also from prolapse and inflammation of the rectum. In the beginning of March, 1884, he began to complain of always feeling sleepy; he fell asleep at all times during the day, and would often say he would die of sleepiness. By the 10th March, it was necessary to confine him to bed, and there he remained more or less completely asleep till the beginning of June. His appearance was that of ordinary sleep; no trismus, convulsions, or contractures; no movements of the eyelids; temperature, pulse, and respiration normal; no failure of general nutrition. He was fed by hand; after a little insistence he opened his mouth, and once the food was placed in the mouth he swallowed it. With one exception, he never passed his dejections in bed; several times a day he was placed on the night-chair when he passed his evacuations, and occasionally he would even make an attempt to get out of bed to relieve himself, but all these attempts were accomplished in an automatic fashion; he never awoke. The patellar reflexes were a little exaggerated. General sensibility was much blunted; pricking with a needle provoked only the slightest movements, though pinching, tickling, or the passage of the Faradic current produced a more lively reaction. The special sensibilities seemed completely abolished; he made no movement when a pencil was brought close up to the cornea, or when loud noises were made near him, and he would swallow aloes placed in his soup without the slightest grimace. The nocturnal sleep did not differ in any respect from the diurnal.

On 3rd April, the temperature suddenly rose, the pulse became rapid, his face red, he became agitated, and, for the first time, he dirtied his bed. The cause of this was found to be an inflammation of the prolapsed rectum, and after that was remedied, he fell again into his old condition.

The awaking was progressive, and by the middle of June he had resumed his ordinary life. Five months afterwards, there were no signs of a relapse, nor was his mental condition in any respect changed.

In the opinion of the writers, this sleep was a purely physiological sleep, but of abnormal duration, constituting a neurosis *sui generis*. They divide pathological conditions of sleep into four groups—the 1st, including those patients suffering from some form of mental alienation, most usually melancholia; the 2nd, the conditions of hypnosis, which occur in hysteria and epilepsy; the 3rd, those associated with well determined cerebral lesions, and some general affections, such as diabetes; and the 4th, those cases in which no pathogenetic reason can be discovered, this last being subdivided into (1) cases characterised by frequent attacks of sleep of short duration (narcolepsy); and (2) cases of sleep of long duration.

*Books, Pamphlets, &c., Received.*

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- Syllabus of Materia Medica. By Alex. Harvey, M.D. and A. Dyce Davidson, M.D. Eighth Edition. London: H. K. Lewis, 1887.
- The Hunterian Oration, delivered 14th February, 1887. By Wm. S. Savory, F.R.S. London: J. & A. Churchill.
- Stricture of the Urethra. With Original Wood Engravings. By E. Distin-Maddich, F.R.C.S. London: Baillière, Tindall & Cox, 1887.
- Inoculation for Rabies and Hydrophobia. By Surgeon-General C. A. Gordon, M.D., C.B. London: Baillière, Tindall & Cox, 1887.
- Indigestion, Bilioussness, and Gout in its Protean Aspects. Part I. Indigestion and Bilioussness. By J. Milner Fothergill, M.D. Second Edition. London: H. K. Lewis, 1887.
- Manual of Bacteriology. By Edgar M. Crookshank, M.B. Second Edition. Illustrated with Coloured Plates and Wood Engravings. London: H. K. Lewis, 1887.
- On the Pathology and Treatment of Spermatorrhœa. By J. L. Milton. Twelfth Edition. London: Henry Renshaw, 1887.
- Von Ziemssen's Handbook of General Therapeutics. In Seven Vols. Vol. VI: Electrotherapeutics. By Wm. Erb, M.D. Translated by A. de Watteville, M.D.; and Vol. VII: Therapeutics of Circulatory Derangements. By Prof. M. J. Oertel, M.D. Translated by Edward J. Edwardes, M.D. London: Smith, Elder & Co. 1887.
- A Practical Treatise on Diseases of the Eye. By Dr. Edouard Meyer. Translated by Freeland Fergus, M.B. With very numerous Illustrations and Three Coloured Plates. London: Charles Griffin & Co. 1887.
- Practitioner's Handbook of Diseases of the Ear and Naso-Pharynx. Third Edition of the Aural Surgery. By H. Macnaughton Jones, M.D. London: J. & A. Churchill, 1887.
- The Commonhealth. By Benjamin Ward Richardson, M.D., F.R.S., London: Longmans, Green & Co. 1887.
- Hay Fever and Paroxysmal Sneezing, with an Appendix on Rose Cold. By Morell Mackenzie, M.D. Fourth Edition. London: J. & A. Churchill, 1887.
- L'Année Médicale, 1886. Publié sous la direction du Dr. Bourneville. Paris: Librairie Plon, 1887.
- The Climatic Treatment of Consumption. By James Alex. Lindsay, M.A., M.D. London: Macmillan & Co. 1887.
- A Course of Elementary Practical Histology. By Wm. Fearnley. London: Macmillan & Co. 1887.

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ORIGINAL ARTICLES.

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THE TREATMENT OF THE INFLAMMATORY AFFEC-  
TIONS OF JOINTS.

By DAVID N. KNOX, M.A. M.B.,  
Surgeon to the Royal Infirmary, Glasgow.

*(Read before the Southern Medical Society, 24th March, 1887.)*

MR. VICE-PRESIDENT AND GENTLEMEN,—The importance of this subject is great, not only on account of the number of inflammatory diseases of joints which come under our notice, but still more because these diseases come so directly under the notice of general practitioners at an early period of their course, and only come under the notice of hospital surgeons when the former have tried in vain to arrest the onward progress of the disease.

Allow me, before entering on the question of treatment, to refer to some of the more general features of inflammatory diseases of joints. In most modern text-books the tendency to refinement in classification of these diseases is very great, and the divisions and subdivisions of the subject very numerous. But we all know that the inflammatory process is essentially the same in all the tissues of the body, being only modified in form and results by certain special features of the tissue affected, such as its anatomical relations, its vascularity or non-vascularity, its softness or hardness, the ease with which it may be reproduced or the



reverse, and by certain constitutional peculiarities or tendencies of the patient which we designate by such terms as strumous, tubercular, rheumatoid, &c. The inflammatory process is also modified by exposure of the affected tissue to the air, especially to air polluted by noxious exhalations from other patients, or to foreign bodies or other persistent sources of irritation, but as these latter conditions will only affect wounds of joints and simple arthritic affections in their later stages, we may in the meantime overlook them.

If we consider inflammation of a joint in the light afforded by the anatomical character of the tissues affected several peculiarities at once strike us. There is the rapidity with which large effusions take place into the synovial cavity; the peculiar shape given to the contents of the joint by this effusion; the position of semiflexion and immobility produced by the contraction and rigidity of the muscles acting over the joint; the atrophy of these muscles, especially on the flexor aspect; sometimes the transference of pain to a distant part; the grating of opposed surfaces from the destruction of the cartilages; the softening of ligaments and the deformity due to altered shape of bones. But the inflammatory process itself, whether it stops short at redness of the synovial membrane or goes on to exudation or to the so-called ulceration of cartilage or destruction of ligaments or rarefaction of bones, or any other termination, is essentially the same process as regards its causation, its results, or the principles of its treatment, as a pneumonia or a pleurisy. The peculiarities noted are not true differences, and they afford no special indications to the surgeon.

Again, if we consider the processes of repair, as they occur in an inflamed joint, we find that they afford but few special indications of the line of treatment to be pursued. Cartilage is seldom or never restored. Bone may be reproduced, but the process is always slow, and in severe cases imperfect, while fibrous tissue is easily reproduced, and generally replaces cartilage, and may replace bone as well. It thus happens that a joint once severely inflamed is permanently damaged, and may even be rendered useless by a perfectly normal and healthy process of repair. Similar results may follow the inflammatory process in many other organs of the body, and consequently they are of little special service here as indicating special lines of treatment. If the surgeon intensify his measures to meet the acuteness and severity of the disease, and if he estimate aright the tendency to deformity, and apply such rigid apparatus as will put the parts thoroughly at rest

and prevent that deformity, he will have complied with all the special indications now afforded him. For the rest, the ordinary process of inflammation, its results and appropriate remedies, are all that he need consider.

On the other hand, when we leave what may be called an ordinary inflammation of healthy or unbiassed tissue, and come to the consideration of the phenomena and results of the inflammatory process in unhealthy tissues—that is, tissues tainted, so to speak, with a decided tendency in a strumous, tubercular, or rheumatoid direction, we find ourselves surrounded by many difficulties, both pathological and therapeutic. The so-called diathesis is not always at first manifest, nor are we even entitled to assume it without evidence. As Professor Gairdner says, “The proof of a diathesis is essentially the proof of disease. It may well be disease in its earliest manifestations and least notable forms.” It is always “an inference either from previous facts in the history of the individual indicating deranged physiological function, or from manifest structural changes, the result of these, whereby we are enabled to establish, but only as a presumption founded with more or less probability on the evidence, the existence of a *tendency* to similar changes, or changes of some allied order, in the future.” This dictum is especially true of the strumous diathesis, and points to a difficulty of no small importance in the path of the surgeon—the difficulty of determining the tendencies or the diathesis. What, for example, is the strumous diathesis? This is a fair question to put, and one rather difficult to answer. Many pathologists are now endeavouring to prove that what the surgeon calls scrofula or struma is merely a localised tuberculosis. They have found in many cases at least, in the tissues of the so-called strumous joint and elsewhere, numerous miliary tubercles, and have come to the conclusion that all such diseases owe their peculiarities to this cause, and that there is nothing else to which we ought to give the confessedly vague name of struma or scrofula. I am prepared to go with the pathologists as far as the microscope shows, but no farther. I will acknowledge that many cases of so-called scrofulous joint are really tubercular, and display all the malignant characters of tubercle, but I hold, and I think most surgeons will agree with me, that there is a large class of cases truly scrofulous or strumous, and yet non-tubercular, or at least that there is no proof of their tubercular nature, and which present a very definite set of symptoms, and are, on the whole, in their earlier stages, fairly amenable to treatment. What I mean by scrofula I would

define somewhat as follows:—Scrofula is a tendency to inflammation on the part of certain tissues of the body, notably the skin, mucous and serous membranes, the lymphatic glands, bones and joints; this inflammation is excited by the most trifling causes, is of a very low aplastic type, is very chronic, is most destructive of normal tissue, and always ends in suppuration and extensive fatty degeneration of the inflammatory products. As this discussion is not a pathological one, I cannot detain you by stopping to defend this definition, if I may call it such. I merely state it dogmatically as representing my own opinion on the subject, and as giving the rationale of my practice.

The clinical history of such cases is very uniform. The joint affected may have received a slight injury within the knowledge of the patient or not. There is at first stiffness, slight pain, heat, and swelling. This swelling is doughy, and follows the outline of the synovial sac. Sometimes there is distinct fluctuation. This condition may last many months without attracting much of the parents' attention, the pains being considered possibly "growing pains," or rheumatic, or something that does not require any special professional advice. The patient is usually young in years—under 25 or so—but not always. I have noted among the cases of strumous joints under my care at the Royal Infirmary, during the last 2 years, five patients: one of 65 with the disease in her knee, one of 64 in the elbow, one of 50 in the knee, and two of 47 and 42 respectively also in the knee. However insidiously the disease begins the surgeon is sooner or later called in, and finds the pain very considerable; the swelling so great that all the natural hollows about the joint are filled up; the skin tense and shining, usually white and marbled with blue veins, and appearing greater than it really is from the wasting of the muscles in the segment of the limb above the joint. The bones are also felt to be enlarged; the joint is fixed or grating, the patient is hot and feverish, wakeful at nights or starting up with a cry of pain—if a child, moaning or even wandering during its fitful sleeps—if an adult, complaining of the startings of the limb. It is at this stage that symptoms of abscess show themselves over the joint. This may arise from suppuration in the joint cavity, the pus bursting through the capsule and seeking an outlet. Or the pus may be entirely extra-articular, arising in the new formation or in connection with the bones. But I need not follow the course of the disease through the formation of sinuses, the destruction of the ends of the bones and ligaments, or the

subsidence of the disease with ankylosis, and more or less permanent deformity. These are features sufficiently well known, unfortunately, to all of us.

Any of the joints of the body may be affected with this disease. During my last two and a half years' practice at the Royal Infirmary, I have had in all 95 cases of the disease as affecting the larger joints. Of these there were 37 cases at the knee, 22 at the elbow, 18 at the hip, 10 at the ankle, 6 at the wrist, and 2 at the shoulder. There were also numerous cases where the fingers and toes were affected, but I have not included them. In a good many cases more than one joint was affected, but there was no evidence of symmetry. In several cases the disease occurred in patients who had rickety curves of the legs or had Pott's curvature of the spine or strumous ophthalmia. In only two cases were there unmistakeable signs of tubercle. The first was a little boy of five years, who was admitted suffering from *morbus coxae*, and who died after excision of the joint from tubercular meningitis. The second was a young man who died after excision of the elbow from tubercular ulcers of the intestine. Two of these had perforated the bowel. After death, tubercle was found in the lungs, intestines, prostate, vesiculæ seminales, and testis. Of these 95 cases, 3 died after operation, 4 refused operation and left the Hospital, and 7 more left *in statu quo*. Of the remaining 81, 40 left the Hospital "cured," and the remainder are marked in the journal as "improved." This is a very vague word confessedly, and I must state what I mean by it. It is impossible, in a general hospital, to retain all these cases till they are completely cured. For this many months or even years would be required, and perhaps not even then would a cure be effected. Not only is room constantly required for accidents and urgent cases, but it is found that after a certain time a further stay in a hospital atmosphere is absolutely harmful to many of these patients. It has therefore been my practice to retain such patients till improvement both in the state of the joint and in their general health was marked, and they appeared to be on the fair road to ultimate recovery. No one, therefore, was dismissed till he had been resident for a very considerable time in the Hospital, and till it was seen that with care and the necessary medicines and appliances only time was required for a complete recovery. Of course, some of these cases come back to us for further treatment, and they were then for the most part worse than before. Generally this arose from the poverty or negligence of the parents to con-

tinue the treatment. The great majority were, however, seen no more.

The lines of treatment followed were, shortly, as follows:—

(1.) *Absolute rest* to the joint in every case. This was secured in most cases by the use of splints, in others by extension. At the same time any flexion or faulty position was remedied under chloroform.

(2.) If the disease were apparently advancing, but no great amount of degeneration had as yet taken place, *counter-irritation* over the joint was employed. For this purpose I used liniment of iodine, fly blisters, or the actual cautery.

(3.) If the disease were comparatively inactive or stationary, *sorbifacients*, especially Scott's dressing, with *pressure* over the joint and a *starch* or *plaster bandage* were used.

(4.) In all cases *cod liver oil* and *iron* were administered in appropriate doses.

(5.) *Nourishing food* of the best possible kinds, especially beef tea or meat soups, with milk. In some cases porter or malt extracts, and even whisky, were of great benefit even to young children. I have not hesitated to give alcohol at times.

(6.) And, lastly, in suitable weather, I always try to get patients out into the open air and sunshine when they are able to walk, and if I had little carriages or other conveniences I would have sent out some of those who could not walk but might have been carried downstairs.

Under such a *regime* it is astonishing how many patients rapidly improve. The pain ceases, and the other symptoms begin to recede. It may take months or even years before a cure is effected, but, given the time and the necessary attention, even in very bad cases, I would not despair of an ultimate cure.

Many of the cases marked cured in my list were submitted to operation. The operations performed were amputation in 9 cases and excision in 21. This latter class includes two partial excisions, where part of the bone only was removed and the interior of the joint scraped out. But into the particulars or merits of these operations I do not here mean to enter, as they do not quite enter into the province of the general practitioner. It is, however, of some importance to him to know in what cases operation should be performed, as the earlier it is seen that the remedial measures mentioned above are of no avail, the better will be the result of the operation. The chief indications for an operation I would state thus:—When, in spite of treatment, the pain steadily

grows worse, the swelling increases from suppuration in or around the joint, the joint is hopelessly disorganised, the temperature constantly fluctuates, and the patient is rapidly losing flesh and strength, the sooner the focus of irritation is removed the better. It is also advisable that the practitioner should be able to say to the patient's friends that the results of these operations are, on the whole, most satisfactory. Of 9 amputations only one died from tubercular peritonitis and perforation, and of 21 excisions only 2 died, one of the hip from tubercular meningitis, and the other of the knee from shock within 48 hours. This last was the only death attributable to the operation itself.

In this class of strumous arthritis I ought to mention that I have included only such cases as could fairly come under the definition with which I started, as well as the two cases which showed themselves to be of a truly tubercular nature. I have also included such cases of strumous disease as were secondary to inflammation of the articular ends of the bone. These cases, however, are of some importance from the point of view of treatment, and I would like to refer to them more particularly. The chief features of this articular osteitis, or osteo-myelitis, as it may be called, are that generally after a slight injury to the end of a long bone, *e.g.*, the femur, swelling, heat, and pain in the bone are complained of, the pain being severe at night; the skin becomes puffy and even reddened and tender; the swelling frequently increases with great rapidity, and may extend up the shaft for a considerable distance. At the same time there is considerable fever, and perhaps rigors, owing to the formation of pus beneath the periosteum. In other cases abscess forms in the interior of the cancellated tissue or epiphysis. In severe cases there may be necrosis of the bone with separation of a sequestrum; in milder cases the bone is only thickened and hypertrophied. This thickening may even be over a limited area and produce a marked unilateral deformity. Sooner or later, in the acute cases earlier than in the more chronic, the joint becomes affected and shows all the phenomena of strumous arthritis. This generally occurs by simple extension from the bone to the synovial membrane and cartilage, but occasionally by the bursting of an abscess through the articular cartilage. The disease is always more rapid than in primary strumous arthritis, most of the cases I have had reaching the hospital within a few weeks of the onset of the osteitis. In all my cases the disease of the bone was more marked than that of the joint. In several there was a large amount of sub-periosteal pus with limited necrosis,

especially of the popliteal surface of the femur. In two there was abscess in the end of the bone. The lower end of the femur is the favourite seat for this form of the disease, and out of 15 marked cases 8, or rather more than one-half, occurred in this situation; at the lower end of the humerus there were 2; at the upper end of ulna, also 2; at upper end of femur, 1; at upper end of tibia, 1; and at lower end of fibula, 1.

The treatment of this disease should be from the first of the most active kind. If seen before sub-periosteal exudation has taken place, the actual cautery is perhaps the best treatment. But if there is the slightest fear that exudation has taken place, or that abscess has formed anywhere, a free incision should at once be made down to the bone and all discharges cleared out. If nothing be found on the surface, or if the bone be swollen and enlarged, a hole should be made into the heart of the cancellated tissue. This will allow an abscess to be drained, or sequestrum to be removed, and will at once thoroughly put a stop to the pain and the spread of the disease. If the neighbouring joint is disorganised it may be necessary to excise or even amputate. Whatever is done must be done with the strictest antiseptic precautions, all the more that there can be no doubt that some of the acutest of these cases are of an infective or pyæmic character, as described by Chassaignac, so that an incision carelessly made or attended to might lead only too readily to what he called osteo-phlebitis and death.

Leaving this subject, I wish to say a few words about rheumatoid arthritis. The subjects of this affection are usually past middle life—at least, over 40. They are all healthy, robust people, and generally with a rheumatic history. The affection may present different forms, but in all is there a marked tendency for the fibrous tissues to be affected and for the inflammatory neoplasm to become organised into connective tissue. In many cases there is a marked tendency to the formation of fluid in the joint, in others dendritic growths are felt after a time, and in some cases loose cartilages may be produced. There is often the history of an injury, though not always, and the disease is extremely chronic, indeed, often well nigh incurable.

Our treatment of such cases should be chiefly constitutional, and directed to the diathesis, the diet, and general habits of the patient. The local treatment will vary with the condition, being sedative or stimulant as required. Where there is thickening round the joint blisters, iodine, or the

actual cautery will be of use. In cases of much synovial effusion I have opened and drained the joint, and so given much relief, and where there are loose cartilages they should be removed.

Gentlemen, I am painfully conscious of many gaps in this rapid survey of inflammatory joint affections, but what I have said may prove, I hope, an efficient introduction to the discussion of the subject. Anything of importance I have omitted will be supplied by other speakers. I have endeavoured to indicate principles as far as possible, and if I have spoken too much of my own work, my apology must be, that it is always of oneself and one's own doings that a man speaks best.

## ON THE RELATION OF THE AIR WE BREATHE TO OUR COMMON DISEASES.

BY FRANCIS HENDERSON, M.D.

(Continued from page 42.)

II. The constituents of the air which are chiefly concerned in the relationship between the inspired air and our common diseases.

We shall use the phrase "common diseases" in a restricted sense, meaning by it "the classified diseases" of the *Vital Statistics of Glasgow*, a list of which was given in the first part of this article. These are classified because they are the common diseases, occur in large numbers (contributing about one-half of the total death-rate), are well known, and therefore afford a broad and comparatively secure foundation for statistical conclusions. We shall entirely exclude from this discussion the zymotic or infectious complaints, and also the "unclassified" non-infectious diseases. By restricting ourselves to the "classified" diseases we shall be dealing with certain specified complaints, regarding which we have already learnt the following important facts:—

(1.) That their frequency in any locality is determined by the quality of the air.

(2.) That all of them are to be met with in every district of Glasgow, under the best as well as under the worst sanitary conditions.

(3.) That when the districts of Glasgow are arranged into groups, so as to bring out as strongly as possible the different sanitary conditions, the remarkable fact is observed—viz.,



each of the classified diseases contributes in nearly the same proportion to the total death-rate of each group, or otherwise stated, when a larger supply of purer air is obtained, the death-rate of each of the classified diseases falls in nearly the same proportionate degree.

We do not profess to understand the full meaning of this fact. So far, it tends to exhibit the paramount importance of material agents in the genesis of disease, in their immediate action as *exciting* causes, and also, it may be, in their earlier and more gradual action as *predisposing* causes.

(4.) Lastly, That the classified diseases occur in all ordinary air, because the material capable of becoming *materies morbi* exists in all ordinary air.

This last conclusion throws valuable light upon the subject we have now to consider.

If ordinary air contains constituents which can become the material agents of our common diseases, then we know that the impure air of overcrowded dwellings owes its great disease-producing power not to the *absence* or deficiency of any ingredient which exists in ordinary air, nor to the *presence* of any essentially new ingredient produced under these insanitary conditions, but to the excess or concentration of certain constituents normally present in the air. For it is impossible to hold—knowing what we do of these diseases—that any one of them can have for its *materies morbi* two different kinds or forms of matter. All will agree that the material agents at work, in hydrocephalus or pneumonia for example, must be the same in kind, whether the case occurs in the best sanitary district or in the *Bridgewater and Wynde* of Glasgow.

But apart from arguments of this sort, it has been proved that neither deficiency of oxygen nor excess of carbonic acid, which are the invariable characteristics of the impure air of crowded dwellings, is *directly* concerned in causing disease. No doubt, *indirectly*, deficiency of oxygen is greatly concerned, because, if present in abundance, it dilutes and perhaps destroys certain other ingredients; but chemists and scientists, among whom may be cited Liebig and Angus Smith, have shown that a considerable diminution of oxygen, provided nothing else objectionable takes its place, is not injurious.\* The blood seems to be able to absorb as much oxygen as it requires from air which contains considerably less than the normal quantity. As regards carbonic acid, in the amount in which it exists in the air of overcrowded dwellings, it

\* See *Air and Rain*. Dr. Angus Smith, p. 191.

probably has no effect. At all events, in much greater quantity, although it may lower vitality, it does not cause disease. Indeed, although when inhaled in a pure or nearly pure state, it rapidly kills, it appears that this is not because it is poisonous *per se*. It kills by taking the place of that which is absolutely essential to life—viz., oxygen. Without going into further details regarding the inorganic constituents of the air, it is sufficient simply to state that the general conclusion arrived at by chemists is, that the causes of disease are not to be found among these, and therefore the connecting link between air and disease must be furnished by the organic constituents.

As the outcome of his own laborious and exhaustive investigations on this subject, Dr. Angus Smith, when referring to the stifling and indescribably disagreeable atmosphere of overcrowded dwellings, says:—"We owe our discomfort to the organic matter." This discomfort is not only due to the impression made by the foul air upon the special senses, it is chiefly due to its effects after being inhaled. If prolonged it tends to merge into disease. Persons living in such an atmosphere, no doubt, cease to notice the discomfort. The external senses and the internal sensations become blunted, but this does not prevent the organic matter producing its natural effects. Adaptation or acclimatisation may take place to a certain extent, the capacity of the blood and tissues to deal with the abnormal amount of organic matter may possibly, like other functions, be augmented by the necessity for its exercise, although the limits of expansion are probably not great. In very vigorous individuals all this excessive organic matter may be broken up and assimilated or excreted and no harm result. In some instances the process of assimilation may possibly be so active, and on so large a scale, as to become nutritive. Such cases have originated the popular saying that "dirt fattens." \* In less vigorous individuals, and still more in persons of feeble constitution, as well as in young children, the assimilation or excretion of excessive organic matter, which reaches the blood with the inspired air, is imperfectly performed, and mischief is liable to ensue. Even in the robust their protection lasts only while their abounding vitality lasts. As age advances or the vital powers are lowered by other causes, they do not escape

\* In this connection the following fact is of interest. Speaking of the examination of air charged with the products of putrefaction, Dr. A. Smith says:—"Fatty matters can be readily recognised, among others, and these require a greater time and more play of the air for their complete conversion into inorganic substances."—*Air and Rain*, p. 477.

the bad effects of impure air. Here is to be found one great reason why the mortality from acute diseases amid insanitary surroundings is so high. The protective powers of the body are so fully engaged in resisting the action of excessive organic matter, constantly present in the air, that the forces in reserve to repel the attack of a new and powerful invader are too often inadequate.

Before proceeding further, it is necessary to distinguish between the two kinds of organic matter which are present in the air.

(1.) There are what may be termed *organic products*, which are principally the result of the decomposition of dead animal and vegetable substances, and include also products given off from living animals and plants.

(2.) *Organised matter*, the seeds or spores of fungi and other low forms of life; in short, the air-germs or organisms regarding the wonderful powers and functions of which much has been discovered of late years, and very much more, doubtless, remains to be discovered.

Now, the question is, which of these two kinds of organic matter supplies *the material* which is chiefly concerned in the production of our common diseases?

In the impure air of overcrowded and insanitary dwellings, both kinds of organic matter exist in large quantities, and therefore no answer to the question comes to us from this side. Again, in the best ordinary air—which, as we have seen, contains a certain amount of this disease-causing material—both kinds are to be found in reduced quantities. The organic products exist in very small quantity, their break-up by the action of abundant air being very complete. On the other hand, the organised matter—although much diminished, as has been experimentally demonstrated by Miguel and others—is still in sufficient abundance to render the track of the solar or electric beam luminous; for that the particles which thus act in scattering the light are living organised matter is proved by their ability to fertilize or infect sterile solutions.\* Therefore, although in good ordinary air the organic matter chiefly exists in the organised condition, still, as there is a certain amount non-organised wherever there is animal or plant life, we cannot ignore this fact in drawing etiological conclusions.

There are other considerations, however, which give a clearer response to the question before us. We have no evidence that the *organic products*—in the quantity, at all events, in which they are present in good air—are capable alone of exerting

\* See Professor Tyndall's *On the Floating Matter of the Air*. 1881.

any influence on the functions of our bodies. Our knowledge of the capabilities of *organised matter* leads to a very different conclusion. It contains the vital principle, implying growth and reproduction, and so possesses powers to which we can place no limit. It is well known that the fluids and solids of the body are suited as soils for the growth of many of the common air-germs or organisms, from the attacks of which they are only defended by their own vitality. When the tissues are denuded of their natural covering, and freely exposed to the air, they soon exhibit evidences of the action of the living particles which air normally contains. Inflammation, suppuration, and even septicæmia may follow.

Still more pertinent and conclusive, as regards the question at issue, is the fact that living organized particles have been actually found in connection with a large number of our "common diseases." Indeed, in at least two instances—consumption and pneumonia—the micro-organisms which are present are considered by the highest authorities to stand in the position of *causal relationship*. They fulfil the conditions which Koch has laid down as essential to their being so regarded.\* Concerning other diseases in which the micro-organisms which are present can be shown not to be *causally* related as defined by Koch, that does not preclude the possibility that some other micro-organisms—which, from their smaller size or for some other reason, have hitherto escaped observation—may be present, and may prove the true *causa causans*. This point is insisted on by Koch, and its importance is illustrated by the bacteriological history of several diseases. But besides this possibility, it must not be overlooked that, although the micro-organisms which are found in the tissues fail to satisfy all the conditions necessary to establish their *causal relationship*, it does not therefore follow that they are non-pathogenic. To complete the scientific demonstration of its causal relationship to a disease, it is certainly requisite that an organism (after entire separation from all extraneous matter by successive cultivations) must be capable of setting up the disease anew when re-introduced into a healthy animal; but this test may fail even when the real micro-organism is being experimented with. A micro-organism may be really *pathogenic*, and yet not so actively or virulently so as to be able to establish itself on perfectly healthy tissue without the help of certain favouring conditions. This is well illustrated by Koch's endeavours to excite cholera by the comma bacillus. Guinea-pigs, in their ordinary condition, are not affected by this organism, even when injected into the stomach; but, as

\* See page 2, *Micro-Organisms and Disease*: E. Klein, M.D. 1886.

proved by Koch's ingenuity, if the stomach is previously rendered alkaline by a dose of potash; and if the natural rate of the peristaltic movement is retarded by opium, then the comma bacillus can grow and produce true cholera.\* In like manner it is reasonable to believe that alterations in the chemical state or in the vital action of our tissues, may enable germs or organisms which may have reached them, and which otherwise would be destroyed, or at least incapable of growth, to grow and multiply. Whenever they do so, certain symptoms and certain further pathological changes follow, of which they must be regarded as the true cause. Organisms so acting are surely entitled to be called *pathogenic*.

Again, let the question be looked at from another side. We hold it proved that the organic matters of the air supply the material causes of our common diseases. Now, if we were in a position to prove, further, that it is the organised or germinal portion of this matter which is the essential part, then it appears to us that there would be no escape from the conclusion, that the common air-germs or organisms stand in a similar relation to our common diseases as the *recognised pathogenic* germs, such as the comma bacillus and the tubercle bacillus, do to cholera and phthisis. These organisms are not apparently capable—at all events when they are introduced into the body by the natural channels and in ordinary quantities—of exerting a pathogenic influence upon a healthy man without the presence of certain favouring circumstances or conditions. Common air-germs or organisms may require still greater facilities, more decided departures from the normal state of the tissues or organs, to allow of their acting *pathogenically*, and they may require to be present in much larger numbers; but these differences are differences of degree, not of kind. The proximate result in both cases is the growing of the respective organisms. Thereafter, disease begins to develop, and its characters are chiefly determined by the specific qualities of the organism, or it may be organisms,† which have obtained a footing.

\* See Address by Dr. Koch at the second Conference on Cholera at Berlin, in May, 1885. (Translated in *Brit. Med. Journal*, Jan. 1886, p. 62.)

† Authorities in bacteriology teach us to regard an abscess as a "circumscribed invasion of micrococci;" that the pain, inflammation, and suppuration are all consequences. There are several species of micrococci usually present, but it has been proved by experiment that some of these species (at least six according to Rosenbach) have each the power, acting singly, of exciting abscess. (See *Micro-Organisms*, Sydenham Society, 1886. Watson Cheyne.) This suggests that in some of our common diseases the material cause may not be limited to one species of organism. Two or more species may grow together, and the symptoms and pathological changes may be due to their combined action.

Such views of the nature of the connection between the air we breathe and our common diseases are strongly supported by what is actually known. They imply that it is the living organised air particles that are the essential ingredients, which is not demonstrable, but it will be generally admitted to be more than probable for the reasons above referred to, and also on other grounds.\* But although the organised matter is alone essential to the genesis of disease, it does not follow that the dead organic products in the air have no influence upon health. In the large amount and offensive condition in which these are present in the atmosphere of overcrowded and non-ventilated apartments, this influence can scarcely be other than injurious. Passing into the blood with the inspired air in abnormal quantity, the organic products must be either assimilated or excreted. The excessive call on these functions must necessarily diminish the ability of the blood and tissues to deal effectually with the more potent organised matter which is often simultaneously in excess. There is also another way in which the organic products are likely to have an injurious influence—viz., by providing a pabulum upon which the organised particles may feed and multiply, until they acquire strength and number sufficient to overcome the vital resistance of the living tissues with which they come in contact. It is thus that the effect of polluted air, in increasing the fatality of the ordinary infectious complaints, is usually explained.

The conclusion, however, to which we are brought is, that the importance of the two kinds of organic matter in relation to the production of disease is very different, and therefore it follows, that when air is impure the relative quantity of each which is present, is a question of great etiological and sanitary interest.

Exact knowledge on this subject is still wanting. Within certain limits, the amount of each kind increases together. When dead animal and vegetable matter is exposed to the air, putrefaction under ordinary circumstances soon sets in. This process is now universally considered by authorities in this department to be the work of micro-organisms, which live and grow at the expense of the dead material. As putrefaction proceeds, and the broken up particles reach a sufficient degree of tenuity, both the organisms and the

\* We refer especially to the increasing evidence in favour of the conclusion that there is *no inflammation without organisms*, taken in connection with this other consideration, that most of our common diseases exhibit more or less the essential phenomena of the inflammatory process.

organic products become diffused in the surrounding air; but in what relative proportion as to quantity depends upon circumstances not yet fully understood.

The rate of growth and multiplication and term of life of the organisms of putrefaction are regulated by the amount of moisture, heat, and oxygen of the air. Thus, if there is little or no moisture, the organisms dry up, and become inert; if moisture is abundant they flourish, but if so abundant that the putrefying matter is submerged, then the organisms (or at least some of the species) languish or die from want of oxygen. Again, if the temperature is below about 36° Fah. the organisms are paralysed;\* from this point to above 60° Fah. they are very active; between 60° and 70° Fah. they grow feeble,† and as the temperature rises they die and become broken up. Lastly, some of the organisms of putrefaction (the aerobes of Pasteur) die without oxygen, others (anaerobes) are killed by it. But this is not all, for it has been shown that all the common organisms of putrefaction are destroyed by the free action of air, and particularly by ozone; although, as their germs are not killed, the process of putrefaction is only arrested for a time.

By this wonderful provision of Nature, putrefaction, the great purifier, is adapted to carry on the work of cleansing under very varied conditions and circumstances, while by a complex arrangement of excitors and inhibitors, its rapidity is kept under control.

To estimate from the above data the relative proportion of the living organised matter and the dead organic products in emanations from putrefying matter and their consequent sanitary importance, is a difficult, if not an impossible task. But these considerations do not exhaust the important elements of the problem. In addition must be reckoned the exact nature of the decomposing material, for this determines to a large extent the species or the qualities of the micro-organisms which it breeds. It is manifest, therefore, that until we possess far more exact and extensive knowledge of all these circumstances and their effects, as

\* Professor M'Kendrick and Mr. Colman have proved that the organisms of putrefaction or their germs are not killed at extremely low artificial temperatures.

† The Rev. Dr. Dallinger has shown, by a long series of experiments, extending over many years, that some organisms become acclimatised to a much higher temperature without losing their vigour, by raising the temperature one degree or less at a time, and this must be done at long intervals.—*Brit. Med. Journal*, 19th February, 1887.

well as of the potentialities of the numerous species of the organisms of putrefaction, it is impossible to make predictions on scientific grounds, as to the influence on health of particular effluvia. We can only arrive at a conclusion on such a question by observing the effects actually produced. While this is true, there are certain circumstances in which practical inferences may be drawn from some of these principles.

When, for example, the process of putrefaction is very rapid, then the organisms themselves perish in the general destruction, and in that case the resulting emanations consist more largely of organic products, and may therefore be regarded as less harmful. In his address to the Sanitary Institute of Great Britain, at its meeting in Glasgow, 1883, the late Dr. Angus Smith, speaking on this subject, said—“When, therefore, you see the Clyde seething with gases of putrefaction, and when you smell it to such an extent that a feeling of loathing is produced, you may remember this, that the work of destruction is going on with a wonderful rapidity, and that the enemies of life are being slaughtered there millions upon millions, never to appear in a similar form, though other generations of them may rise up.” On the other hand, when putrefaction goes on more slowly—say from a fall in temperature, and when the stench is much diminished, are we not entitled to infer that the “enemies of life” are not being so largely destroyed, but are passing into the air, which thus becomes more heavily charged with dangerous constituents?

It has been argued that because the Glasgow death-rate is lowest in the summer and autumn months, when the river is most offensive, that the pollution of the Clyde has no injurious effect upon the health of the city. Such a conclusion is certainly not warranted, and may prove to be entirely wrong. We know, at all events, that the disease-producing power of such emanations is not in direct ratio to their offensiveness. It may turn out, and it is certainly more probable, that it is in the colder months that the state of the river is most injurious, and it is in these months that the death-rate of Glasgow is highest from causes, be it observed, which in the opinion of the Medical Officer of Health, have not yet been adequately explained.

Again, it is advanced as an argument for the innocuousness of the state of the river, that putrefying sewage is comparatively harmless when covered with water, because



it is shut off from the action of air. As a general statement this is quite true, but there are facts in this connection which have not received the attention they deserve. The worst portions of the sewage are the lightest in specific gravity. In point of fact, a large proportion of faecal matter, which is believed to be the very worst, floats on the surface of water, and that which may not be quite at the surface is pretty freely exposed to the air by the very constant agitation of the water in the Harbour, by the winds, tides, and unceasing traffic. Such considerations *prove* nothing, but they strengthen the suspicion that the state of the Clyde is injurious to the health of the city, and thus supply additional reasons why this question ought to be thoroughly investigated by those who are responsible for the public health.

With our present knowledge, the influence of organic emanations upon the sanitary quality of the inspired air can only be definitely settled by observing the effects produced. When such emanations are confined within a limited air space their effects are easily recognised, and the degree of these effects can even be approximately measured by determining by experiment how much air is needed to neutralise their injuriousness. House-accommodation in relation to death-rate, may be viewed as a vast and continuous experiment of this kind, and its results are so uniform that they may be accepted as approximately accurate. These are recorded in *The Vital Statistics of Glasgow*, where the ratio between death-rate and air space is clearly exhibited. There also we learn this most important fact, that the amount of air in the houses of the best districts of Glasgow, is not sufficient to reduce, by dilution or oxidation, the organic constituents of the air to the normal proportion, the proof of this being that the death-rate from our common diseases remains above the normal. What is the explanation of this fact? In the opinion of the Medical Officer of Health, it is that the *outside air* is below the normal standard of purity. "The condition," he says, "of the general atmosphere of the city fixes the maximum of purity attainable in the domestic atmosphere." We naturally inquire, What are the causes of this condition of the general atmosphere? Those specified are "smoke, chemical vapours, and the exhalations from our putrid watercourses." Of these, the smoke and chemical vapours are no doubt highly important. They draw largely upon the oxygen, which would otherwise be available to

oxidise and destroy a certain amount of the other impurities, and smoke itself, especially the black smoke of the incomplete combustion of coal, has been shown to contain a certain quantity of organic products. Moreover, smoke, when in large volume, greatly interferes with atmospheric diffusion, upon which the renewal of the air so largely depends. But if we are correct in our conclusion that it is the living organised constituents of the air which supply the material causes of our diseases, then it is the exhalations or emanations containing these forms of matter which, by contaminating the open air, are *directly* concerned in causing that part of the Glasgow death-rate which is due to the condition of the general atmosphere.

With regard to the principal causes or sources of impurity in the air of overcrowded and ill ventilated houses, where a high death-rate from our common diseases is constantly maintained, a few remarks may be offered. The most recent contribution to this subject is a valuable paper in the *Philosophical Transactions of the Royal Society of London*.\*

The important and novel feature of this investigation is, that it includes an extensive and laborious examination of the air of dwelling-houses, schools, &c., for micro-organisms, and exhibits the relation as to numbers which they bear to the carbonic acid and the oxidisable organic matter.† The paper contains many highly interesting and important observations. Among them we observe that in the air of overcrowded houses the increase in the micro-organisms was found to be very much greater than the increase in the organic products. As to the source of these micro-organisms, the result arrived at is, that they are not immediately derived from persons, and only to a comparatively small extent from clothing. The house itself supplies them; it is the breeding place. When one-roomed houses are

\* "The Carbonic Acid, Organic Matter, and Micro-Organisms in Air, more especially in Dwellings and Schools." By Prof. T. Carnelley, Mr. J. S. Haldane, and Dr. A. M. Anderson.

† It must be particularly noted—and the authors draw attention to this point—that the micro-organisms estimated in the several atmospheres compared, were those *only* which are capable of growing on a special medium—viz., Koch's jelly, at ordinary temperatures. Judging from the number of micro-organisms given, these must have formed a very small proportion of the whole number present. It is manifest, therefore, that until extensive investigations of this kind are carried out under very various conditions as to cultivating media, temperature, &c., any final conclusion is impossible.

divided into "Clean," "Dirty," "Dirtier," and "Very Dirty," the number of organisms increases on a very rapidly ascending scale.\*

Many years ago (1846) the late Dr. Angus Smith showed that in crowded rooms the organic matter exhaled with the breath became deposited upon the walls, windows, furniture, &c. In relating the results of his investigations to the British Association, he said:†—"If it (the organic matter from the breath) be allowed to stand for a few days (about a week is enough), it will then show itself more decidedly by becoming the abode of small animals. These are rather to be styled animalcules, and very small ones certainly. . . . They can be seen with a good microscope." In other experiments, he describes the organic matter as being converted into *confervæ*, "as it probably would have been converted into any kind of vegetation that happened to take root." For the removal of this organic matter, Dr. Angus Smith found that free play of air was necessary, or proper washing or hard rubbing, such as experience has shown to be requisite for removing the musty smell from the furniture of an inhabited room. In overcrowded and dirty dwellings, such means are rarely or never applied, and the organic matters collect and can be easily observed on the furniture and other non-absorbent surfaces, as a sticky, bad-smelling coating. This organic matter, it must be observed, is of a complex nature, consisting of the waste products of the tissues of our bodies, that are exhaled from the lungs, and there is reason from analogy for believing that the micro-organisms which will breed upon it will be of a kind not unsuited to grow upon the living tissues, and indeed, their cultivation and re-cultivation in such a medium probably increases their capacity for so doing.

It appears probable that it is in this direction we must look for the explanation of the remarkable morbid activities, as well as of the peculiar qualities, of the air of houses habitually

\* In the above paper the authors state that they found a larger number of micro-organisms in *old* schools, and further, they made the observation, that ventilation by mere diffusion did not much reduce their number, indicating that in old overcrowded apartments there are numerous and long established *colonies*, the prolific powers of which are not at once affected by air admitted by ordinary methods. One is reminded of the history of old hospitals (before the days of antiseptics), which appeared to harbour, if not to breed, the poisons of erysipelas, pyæmia, and gangrene. These considerations suggest an argument from analogy in support of the view that common air germs or organisms stand in a similar relation to the "common diseases" as infectious germs do to infectious complaints.

† *Air and Rain.* Dr. R. Angus Smith, p. 391.

overcrowded and never thoroughly cleansed. No doubt, in such dwellings there are other sorts of putrefying substances yielding different kinds of organic products and micro-organisms, particularly there are excretal matters; but it must be remembered, that the pollution of the air from this latter source, declares itself by the development of a special class of diseases—viz., the so-called filth diseases. Now, it is the distinguishing character, as we have seen, of the air, resulting from overcrowding, that *all* our common diseases increase, maintaining while so doing the ordinary relation in frequency to one another.

We have next to consider a very important question.

We have seen that the material causes of our diseases are derived from the air we breathe, and that their prevalence is in direct proportion to the amount of the material causes. Again, we have seen that it is the organic matter of the air which is concerned in this relationship. Further, we have seen that strong reasons exist for the conclusion, that although the dead organic matter, if in excess, is indirectly injurious to health, it is the living air germs or organisms which are essential to this relationship. The question which now demands consideration is—Do the common air germs or organisms normally enter our bodies with the inspired air? If they do, then their physiological effects must be important and unceasing, and in normal quantity, no doubt, beneficial; but it is also clear that their opportunities for pathogenic action will be manifold, if either they are in excessive numbers or if there is failure or disorder, from any cause, of the vital powers or normal functions.

On the other hand, if the common air germs or organisms do not normally enter our bodies with the inspired air, then it follows, that one of the first steps in the departure from health, which proves to be the beginning of disease, must consist in the breaking down of the barrier which the lungs present to the passage of air germs into the blood.

We are not aware that any completely established facts or crucial experiments yield a decisive answer to this question. We propose, therefore, briefly to state those facts and arguments which, in our view, are most deserving of attention as bearing upon this interesting and important subject.

*(To be continued.)*

## CURRENT TOPICS.

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UNIVERSITY OF GLASGOW.—The following candidates have passed the first professional examination for the degrees of M.B. and C.M.:—John W. Adam, Thomas P. Anderson, Alexander Andrew, Charles Bannatyne, Robert Bell, Arthur H. Browne, James S. Buchanan, John Calderwood, James D. Campbell, Herbert Cane, John Cunningham, Evan Davies, John Dunlop, John Frew, Andrew F. C. Gilmour, Andrew Goldie, Albert A. Gray, James Hill, G. Scott Jackson, John M. M. Kerr, George Lamb, John Lithgow, David Lloyd, James P. Low, Robert A. Morton, James Niven, William Park, Peter Paterson, Archibald Shaw, Alexander R. Smith, Alexander Smyth, Alexander Waugh, Alexander D. Wilson, Hugh C. T. Young, Robert Miller.

The following have passed the second professional examination:—James G. Andrew, Arthur A. Beale, Matthew Blair, James Boag, John Brown, Leslie Buchanan, William Cairns, John Clarke, Thomas C. Craig, Joseph H. Dickson, Alexander R. Fraser, M.A.; John Galloway, James A. Gentle, James Gillespie, Robert Girdwood, Joseph N. Glaister, James Hudson, William Hutchinson, Arthur J. Hutchison, M.A.; John T. B. Laverick, James M'Kee, Joshua B. M'Lean, Charles N. Macquarie, Andrew Morton, Andrew Moyes, David Muir, William R. Muir, Alexander Patterson, Oswald Rees, Hugh S. Russell, James A. N. Scott, Robert Stobo, Lewis R. Sutherland, Andrew S. Tindal, Alexander Watt, William M'Gregor Young.

The following have passed the third professional examination:—John Adams, Matthew Beattie, John F. Boa, James Brown, James R. Dalrymple, William C. Downs, Harry Findlay, Thomas Fleming,\* Hugh Girvan, Hugh Hight, William R. Jack,\* Hugh Jones, Henry L. G. Leask, John Livingstone, Andrew N. M'Gregor, James M. Macphail, M.A.; James T. C. Mitter, Robert P. Shearer, John Smith, M.A.; John Somerville, James Stevenson, James A. Thoms, Randolph O. Willis, Henry J. Younger, William Diamond.

GLASGOW MEDICAL CHARITIES.—The Glasgow Medical Charities Committee met on the 7th inst., in the rooms of the Charity Organisation Society, the members of the Medical Relief Committee of that Society being present. There was a large attendance, and Mr. J. Cleland Burns, the Chairman of

\* Those marked with an asterisk have not been examined in Pathology.

the Council of the Charity Organisation Society, presided, at the request of the meeting—the president of the Charities Committee, Professor Morton, having been detained some time at a meeting of the Directors of the Royal Infirmary. Apologies for absence were received from Professors Gairdner and Sir George H. B. Macleod, Mr. Hamilton, the Chairman of the Board of Directors of the Western Infirmary, Dr. Brown, and other medical practitioners. Amongst those present were Mr. McClelland, chairman of the Govan Parochial Board, and Drs. Buchanan and Muir, members of the Barony and City Board, also Professor McCall Anderson, Drs. Patterson, Pirie, Maclean, Richmond, Clark, McGregor Robertson, &c. The chairman stated that the meeting was convened for the purpose of receiving a report prepared by a sub-committee of the Glasgow Medical Charities Committee with respect to the means adopted throughout the country, in the way of exercising discrimination in the admission of patients to the benefits of charitable medical institutions. Dr. T. F. Gilmour, the convener of the sub-committee, then read the report, which embraced about 100 institutions, consisting of general and special hospitals and dispensaries, cottage hospitals, provident dispensaries, and a few endowed establishments. Some of the reports received were so meagre and unsatisfactory as to suggest an apparent indifference with regard to the rights and interests of subscribers, but very many were model documents, their contents being complete and well arranged, exhibiting the operation of the charities in every light in which they can be set for information, comparison, and criticism. Some of the items in these reports, nevertheless, are not free from the suspicion of abuse of medical charity, by those whose sole duty it is to administer it carefully. Of all the objectionable forms of abuse from which medical charities suffer, or can suffer, the most objectionable is that of the wolf in sheep's clothing, and the following is an example:—The rules of a certain medical charity set forth that the appointments of the honorary surgeon and physician shall be permanent, and that they, with the honorary secretary, shall be, *ex officio*, members of the committee of management, that three shall form a quorum, that meetings of the committee may be called by the honorary secretary at any time, that the committee shall have power to make such bye-laws as they may think necessary, and alter and amend the same as occasion may require. The wonder is that such a charity is not without its due list of patrons, trustees, and subscribers. One of the most salient

points in the reports was the evident complacency with which statistics were submitted of the increase of patients as forming a claim upon the public for contributions. A detailed statement was given of the rights and privileges of donors and subscribers, with respect to the admission of patients and the general management of the hospital's affairs. A noteworthy feature of this part of the report of the committee is, that in the institutions where most rigid discrimination has been exercised as to the suitability of the patients from a social as well as from a medical point of view, no diminution of the funds has taken place. It is pretty evident that the vast majority of subscribers to medical charities are animated purely by motives of philanthropy, and place little value upon the rights and privileges offered, or, in many cases, almost thrust upon them. In several instances lines of recommendation from subscribers, after being in use for many years, have been discontinued. There was no instance of a contrary procedure. Patients considered inadmissible included those able to pay; paupers, if not paid for by the parochial boards; domestic servants and apprentices in place, if not paid for by employers; soldiers in some cases, if not paid for by commanding officers; members of clubs and benefit societies, unless the cost is defrayed; and police cases, unless paid out of public rates. The various modes of discrimination adopted referred mostly to the in-patient departments. The information obtained regarding out-patients was of a loose and imperfect character, as a general rule. This was regretted, as the greatest amount of abuse of medical charity is found to exist with respect to out-patients. In every instance, the growth of the out-patient departments of the charities had been by leaps and bounds bearing no relation to the growth of the in-patient departments, nor to the growth of population, and other local circumstances. While all the reports recognised the duty of attending to individual cases, only a few attached any value to the other and no less important duty to the public of administering the funds for the benefit of fit and proper recipients. The medical officers in many cases decided by examination of the patients who are suitable for admission, but sometimes their power is limited to deciding whether the disease is a proper one for treatment by the Charity. The duty of investigating the circumstances of patients is relegated in a number of small charities to the house porter. In a considerable number of institutions a meeting of the Board of Managers takes place weekly, to admit, refuse, and discharge patients, but in such

cases the medical men are *ex officio* members. No returns have been published to show to what extent patients were refused admission. In some children's hospitals the duty of admitting or rejecting patients was thrown upon the lady superintendent. In a good many institutions a certificate was required from a local medical practitioner to the effect that the applicant was a proper case for treatment. In a large number of charities, the applicant must answer questions on a printed schedule, as to age, whether married, trade, wages, number in family, length of time ill, whether member of benefit society, or in receipt of parochial aid. The most important and successful means of checking the abuse of medical charity were found to consist in careful registration and strict investigation. The registration was done by hospital authorities, and the investigation by the same agents aided by special officers. All applicants were admitted when they first presented themselves and treated, pending enquiry, the results of which were reported to the superintendent of the charity. In towns where Charity Organisation Societies exist, their aid was obtained in the way of investigation. To some hospitals, provident dispensaries were attached to which patients were referred whose means were found to be sufficient to enable them to afford small weekly or monthly payments. Other modes of exercising discrimination were detailed, and the subject of the admission of paying patients was treated of. Dr. W. L. Muir, member of the Parochial Board of the City Parish, supplied very important and interesting information with regard to the medical relief given under the poor law in that parish. Prior to February, 1886, it was the custom to grant medical relief to every one who applied, the medical order remaining in force for the period of three months, and being renewed without question on application. No investigation was made into the circumstances of the applicants, and the medical officers had sometimes to complain of orders being granted to persons who ought to have been paying for medical attendance and medicine as private patients. Such indiscriminate medical relief was often the prelude to an application for ordinary parish relief, and so sapped the independence of the applicant. The average annual number of medical orders during ten years prior to February, 1886, was 2,646. Since that time, indiscriminate medical relief has been stopped, the medical orders granted being in force only for fourteen days, during which period strict investigation is made, and all unsuitable cases are at once cancelled. The number of applicants from



February, 1886, to February, 1887, has been 1,522. Nearly 200 of the orders given to these have been cancelled on investigation, from causes such as not suitable for relief, wrong information, not found at address given, &c. &c., leaving 1,322 deserving cases, or only half of the average number where no investigations were made. No case of hardship has, so far as is known, occurred under the present system. The thanks of the meeting were accorded to the compilers of the reports, and after discussion, the sub-committee, consisting of Drs. Erskine, Gilmour, and Robb, were requested to obtain communications from all the medical charities in the country not included in the report, and to direct special attention to the points bearing upon the modes of investigation and discrimination employed. On the motion of Mr. H. E. Clark, a vote of thanks was awarded to Mr. Cleland Burns for his kindness in presiding, and the Charity Organisation Society was thanked for giving the use of their rooms for the meeting.

THE "SWORD STRETCHER."—Dr. Naismith, whose services to the Ayrshire Yeomanry Cavalry as instructor in ambulance drill for mounted troops were specially noticed by the inspecting officer, Lord Ralph Kerr, at the recent review of the regiment, has improvised a stretcher made with four swords and a piece of canvas. The swords, two on each side, are fastened at the centre of the stretcher by two simple clamps, which are, when not in use, carried in the pouch belt. The piece of canvas, when folded, is buckled behind the cantle of the saddle, after the manner of the valise.

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## CORRESPONDENCE.

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*To the Editors of the Glasgow Medical Journal.*

SIRS,—I shall be obliged by your allowing me to point out a slight inaccuracy in Dr. Gemmell's contribution to Dr. Finlayson's *Clinical Manual*. In discussing the subject of venous tracings, Dr. Gemmell refers to some published in a paper by me some years ago, and in his remarks, on page 694 of the second edition, he writes as follows:—

"The wave 'b' is due to the contraction of the right ventricle, and the wave 'c' in the descent line is said by Dr.

Gibson to be reflected from the interior of the heart, in the same way as the dicrotic wave in the radial tracings of very free aortic regurgitation, as was first pointed out by Naumann."

This explanation of the interruption in the curve of descent was originally given by the late Professor Friedreich of Heidelberg, and, in the paper already referred to, I mentioned this circumstance, which has, notwithstanding, been overlooked by Dr. Gemmell.—I am, Sirs, your obedient servant,

G. A. GIBSON, M.D.

17 ALVA STREET,  
EDINBURGH, 15th July, 1887.

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## REVIEWS.

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*The Treatment and Utilisation of Sewage.* By W. H. CORFIELD, M.A., M.D. (Oxon), Professor of Hygiene and Public Health at University College, London, &c. Third Edition. London: Macmillan & Co. 1887.

SIXTEEN years have elapsed since the publication of the second edition of this work. It is, therefore, full time that the results of further experience regarding the treatment and utilisation of sewage should be collected and estimated.

Whatever progress has been made during this long period in the direction of scientific discovery or of practical methods of application, there can be no doubt that there is very considerable advance in public appreciation of the importance of the subject. The steady fall in the death-rate of towns according as the primary conditions of health are more and more nearly attained, is attracting attention and gaining public confidence. Foremost among the means to secure pure air and pure water is the efficient removal and disposal of sewage, understanding by the term "sewage," as our author does, not only excretal but all refuse matter.

In the present edition—which, with the assistance of Dr. Louis Parkes, has been in great part re-written and considerably enlarged—"the historical portions have been retained in their entirety, as being not only interesting in themselves, but also, on the one hand, descriptive of a state of things still to be found in many places, and, on the other, important as a record of methods and processes which have been adopted at various

times." After giving numerous illustrative proofs that filth and disease stand in the relation of cause and effect, our author proceeds with a detailed description of midden-pits and cesspools, &c., noticing their various improvements and modifications both in this country and on the Continent. They are all condemned, as they fail to fulfil the first condition, that sewage and refuse matter should be at once removed from the house and its immediate vicinity. The least objectionable, from a sanitary point of view, is the "pail system." Its advantages are that, under favourable circumstances, it may be made to pay expenses and perhaps yield a slight profit; and inasmuch as the pails must be emptied daily, it avoids the storage and accumulation of excrement, which is always a source of danger to health. But the pail system is nasty and troublesome, and every householder, in whatever station in life, detests the frequent visits of the scavenger.

The "Dry Closet" systems are next described—the "Ash Closet," "Charcoal Closet," and "Dry Earth Closet." The general conclusion is, that they have a sphere of usefulness in camps and villages, &c., which are not provided with a general system of sewerage. They go on the wrong principle of retaining about the house, instead of getting rid as soon as possible, of refuse matter. They require care to prevent their becoming a nuisance, which occurs if the earth is too small in quantity, too moist, or of bad quality, or the compost by any chance becomes wetted. Besides, although earth, charcoal, &c., deodorise, it is not certain that they destroy noxious emanations. Offensive smell is Nature's hint that the offending matter should be at once removed. If you destroy the bad odour, you may be removing the danger signal but not the danger itself. Again, where there are sewers, as in all towns there must be, for the conveyance of other liquid refuse, there is no good reason why the solid excrement should not also be discharged into the sewers. It is not in itself a great addition, nor does its presence entail what would not otherwise be required, viz., the purification of the sewage, because the liquid refuse (which includes the urine) needs purification before it ought to be discharged into a river.

The next chapter (V) treats of Water Closets and their Water Supply. The various forms of closets—those adapted, by their simplicity and cheapness, for the lower classes of the population—and the result of experience of their use, are described in detail; also the defects of water closets, their dangers, and the injury to health to which they have given rise, are fully considered and illustrated. Professor Corfield clearly shows

that these serious drawbacks are in no way inherent to the water closet system. They are due to faulty construction, careless use, and want of supervision. A thoroughly ventilated, trapped, and flushed water closet is perfect in theory, and may be made perfect in practice. Its comfort and amenity is such that the universal opinion is that no other known plan would now be accepted by the public. The verdict in favour of the water closet system is confirmed by the fact, of which there are abundant statistical proofs, that it has been the means of greatly reducing the death-rate in those towns where it has been introduced. If this has occurred in the past, notwithstanding all the imperfections of the system and the actual mischief which it has occasioned, what may we not look for in the next decade, when Sanitary Protection Associations, Certificated Plumbers, and public official control, may be expected to be abroad in the land?

Chapter VI deals with sewerage—the material of which sewers are constructed, the proper gradients to avoid sewage stagnation, the best modes of ventilation and of flushing, &c. Our author advocates *impervious sewer pipes* and *pervious drain pipes* deeply sunk, the latter to carry off the surface and subsoil water. It is interesting to note the effects on health from subsoil drainage, particularly the remarkable reduction of the death-rate from phthisis, a quite unexpected result. "It appears that in such towns as Salisbury, Ely, Rugby, Banbury, and so forth, where, as we have already seen, the drying of the subsoil has been considerable, the deaths from consumption have been reduced by a third or a half of their number."

Properly constructed water closets, adequate water supply, properly engineered sewers, can be made thoroughly to carry out the principle that excrement and all liquid refuse, should be at once removed from the house, and as rapidly as possible out of the town or city itself. So much for the *removal* of sewage.

Next comes the *disposal* of sewage, which is *the* great difficulty. Professor Corfield entirely objects to the discharge of sewage into water-courses. By such a plan "it is worse than wasted." Rivers become sources of emanations injurious to health, and the water supply, which is the common property of the inhabitants of the districts through which they flow, is polluted. Besides this, there is an actual loss of valuable matter. Our author puts as the very lowest estimate of the annual value of London sewage, one million pounds sterling.

The means of sewage disposal we find arranged under the

three heads, Filtration, Precipitation, and Irrigation. Our space does not permit of our referring to the improvements in detail which have been effected, but the striking fact must be noted that after sixteen years there is no *new* and approved method to add to the list, notwithstanding all the study and ingenuity which has been expended on the subject. Surely this suggests an important lesson—viz., the unreasonableness of Local Authorities of cities, and the remark applies to our own city, in delaying to deal with the question of the purification of their sewage, in the hope that some new discovery may supply them with an easier and a cheaper method.

The perusal of Professor Corfield's book has impressed us with the extent and completeness of his knowledge of the subject. The work is full of information. The opinions expressed are entirely free from dogmatism, and in most cases the reasons for them are fully given, and the conclusions arrived at upon debatable points seem to us extremely judicious. The work is eminently practical, and is, indeed, what the previous edition was entitled, "a digest of facts." Besides being supplied with an excellent index and a very full table of contents, there are marginal descriptions of the text which facilitate reference. In conclusion, we consider Professor Corfield's book admirable, and to be regarded as a standard work on the subject of which it treats.

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*Hay Fever and Paroxysmal Sneezing; their Etiology and Treatment, with an Appendix on Rose Cold.* By MORRELL MACKENZIE, M.D.Lond. Fourth Edition. London: J. & A. Churchill. 1887.

IN the preface to this new edition the author points out the fresh matter which has been added to the work, particularly his experiments to determine the amount of pollen present in the air, the employment of cocaine in the treatment of hay fever, and the appendix on paroxysmal sneezing.

Turning to the book itself, we find that the author first reviews the literature of his subject and shows how, amid varying opinions, the fact became established that hay fever is really due to the pollen of plants acting on the mucous membranes of the eyes, nose, and air passages of susceptible persons. The susceptibility here is a marked example of "idiosyncrasy," and Dr. Morrell Mackenzie proceeds to make a vigorous defence of the use of that word, giving many and interesting examples of cases in which persons have been

made seriously ill by seeing cats, smelling roses, eating eggs, &c.

It is curious to find that hay fever is practically confined to the Anglo-Saxon race, the British and American nations being its chosen victims.

It is also curious to find that it is essentially a disease of the cultured classes, and that it is much more common among men than women! A peculiar nervous susceptibility is at the root of the matter, but the exact nature of this nervous susceptibility cannot be determined. Another peculiarity is that the affection is common among townspeople and rare among the rural population, although the latter are more frequently and closely exposed to the exciting cause. Dr. Morrell Mackenzie does not know whether this is to be attributed to original insusceptibility or an acquired tolerance.

The influence of "sex," "heredity," "age," "heat," "light," &c., are considered, and dismissed, and the author remarks:—"Blackley's observations leave no doubt that the cause of hay fever is *the action of pollen on the mucous membrane.*" Dr. Morrell Mackenzie's confirmatory experiments are very interesting, but the reader is referred to the book itself.

An amusing instance of extreme susceptibility is furnished by the case of a young lady who had an attack after looking at the picture of a hayfield, by Vicat Cole, in the Royal Academy! As regards *treatment*, various collyria have been recommended for the irritation of the eyes, but our author says:—"I have found a lotion containing two grains of acetate of lead with two drops of dilute acetic acid in an ounce of water the most soothing application." When out of doors ample spectacles and a broad brimmed hat are recommended, and a veil when driving. The *nose* should be treated with cocaine spray, and the nasal bougie should be employed. "Tobacco smoke also, sometimes, gives relief." The electric cautery may be used.

The author says:—"For the *relief of the respiratory symptoms* we are able to do much, for the *cure*, unfortunately, very little." Among the remedies mentioned are "the fumes of nitrated blotting paper," "a patent American remedy, consisting of nitrate of potash and powdered herbs, of which stramonium or datura tatula is probably the most important," and "cigarettes of pure stramonium." Palliative treatment consists in leaving the locality where pollen is prevalent and taking a sea voyage, or, failing that, residence at the coast. In the matter of "nerve tonics," Dr. Morrell Mackenzie says:—"I have found valerianate of zinc, in combination with asafœtida, more valuable than any other drug." In confirmed

cases recourse may be had to opium, and belladonna may be tried, "more especially in those cases in which there is irritation of the uvula and roof of the mouth." Stimulants should be cautiously employed. "Tea and coffee are extremely useful in the asthmatic form of the disease." The chapter on "Paroxysmal Sneezing" is very entertaining. The cure for this teasing affliction is *snuffing*, but, as the habit may become confirmed, it is possible that the cure may prove worse than the disease.

The Appendix contains some curious information regarding "rose cold"—a form of coryza caused by breathing the exhalations of roses.

In conclusion, it may be said that this little book is interesting and very readable; a pleasant vein of humour runs through it, and one is instructed and amused at the same time. The work is beautifully got up, and it is furnished with an illustration showing the grasses which most commonly excite hay fever.

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*A Course of Elementary Practical Histology.* By WM. FEARNLEY. London: Macmillan & Co. 1887.

WE have in this book an illustration of the good work that may be done upon a special subject by one who is at the same time engaged in general medical practice. The author has evidently made himself familiar with most of the recent advances in histological processes, and he describes them, on the whole, clearly and succinctly.

The book is divided into chapters, each dealing with a branch of the work, and describing firstly the apparatus required, then the modes of obtaining and preparing tissues for examination, with finally a number of pages with blanks for drawings and descriptions of the preparations.

The first chapter, on microscopy, is not very satisfactory, for while the author mentions the microscopes that may be used, there is no description of the general optical principles of the microscope, nor any indication as to the points that should guide the student in the choice of a good instrument. There is no reference, on the one hand, to the acknowledged superiority of the English high power lenses and methods of illumination over those manufactured on the Continent; nor, on the other hand, to the greater durability and better mechanism of the moderately priced continental instruments, or to the exceedingly cheap and serviceable students' microscopes

recently introduced by Reichert, Leitz and others. Of Abbe's condenser, of apochromatic lenses, of the eye piece micrometer there is no mention, nor are directions given as to the mode of working with the microscope—as to the use of the draw-tube, of the diaphragms, of the low and high powers, of focussing and the like.

The list of re-agents and apparatus is fairly complete. We miss chloride of palladium for decalcification of bone. By the way, is Koch's hypodermic syringe so called "because it can be taken to pieces and sterilised by heat," or because it was invented by the distinguished bacteriologist? We cannot agree that labels should be dispensed with—our index books are not always at hand.

The full details as to injecting should render lighter that *cruz* of the young histologist.

In the list of preserving re-agents we have described the method of hardening with alcohols of increasing strength, but no mention of Betz' method of hardening masses of nerve tissue with iodine, alcohol and potass. bichrom., nor of Fleming's valuable fluid.

We fear that the author's method of removing gum from sections will be rather destructive to delicate specimens, more especially when they have to be cut in large numbers for class purposes. There are no directions as to how sections impregnated with paraffin should be made to adhere to the glass.

The general remarks on staining are good. Reference might have been made to the method of staining with chloride of gold devised by Wolff, which has given such beautiful results in the study of nerve endings. The author makes a decided mistake when he says of iodine-green that alcohol removes the stain very little, so that the sections must not be over stained. On the contrary, the staining must be deep, or the sections will be decolorised in the process of dehydration with absolute alcohol. Bismarck-brown should have been added for photographic purposes.

In speaking of turpentine as a clearing agent, its value in showing nerve fibres, *e. g.*, in the spinal cord, seems to have been overlooked, and it is surprising to find glycerine entirely omitted from the list of clearing re-agents. However, if we follow his rule of never attempting to mount in glycerine without using a cell, we will not learn much from the specimens with our oil immersion lenses.

The list of tissues to be taken from each animal is judiciously made up, and will prevent much waste of time and material.



The size of the book might have been much reduced by leaving out the blank spaces for definitions, and it seems rather a strange way of acquiring knowledge to write down descriptions of an object before beginning to examine it.

On the whole, the book is carefully written—though we meet with such expressions as “*flatted*” glass, “*front lens*” for objective, “photographing microscopic *objectives*,” “air gas burner,” “several lots of tap water,” &c.—and well deserves careful study.

It marks a distinct advance in the field of histological research.

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*Practitioner's Handbook of Diseases of the Ear and Nasopharynx.* (Third edition of the *Aural Surgery*.) By H. MACNAUGHTON JONES, M.D., M.Ch., M.A.O. (Hon.) London: J. & A. Churchill. 1887.

OF new books upon Aural Surgery there has been in recent years certainly no lack, and “the cry is still they come.” We are of those who believe that the superabundance of aural text-books will be distinctly advantageous to the profession, although we are not so sure about the financial profit to the authors. The large supply may be looked upon as a legitimate response to the growing interest in the subject now manifested by students and practitioners, and the result of this literary activity is seen in a large number of excellent English works on Diseases of the Ear.

This book of Dr. Macnaughton Jones does not, of course, claim to be an exhaustive treatise. It is essentially a practical work, written in the clear, interesting style characteristic of the writer, and very well suited for satisfying the requirements of the general practitioner. Like all the author's literary productions, it has the stamp of individuality, and is no mere dry compendium of facts. The first ten chapters are devoted to statistical, etiological, and anatomical considerations, also to general symptomatology, methods of examination, and aural therapeutics. The subsequent chapters are taken up mainly with descriptions of the affections of the various sections of the ear. The author in these descriptions avoids tediousness and repetition, while the treatment recommended is such that most practitioners will be able to carry it out. The diseases of the naso-pharynx, a region which may be truly regarded as a part of the aural apparatus, are fully and ably treated, and the information is quite up to date. The therapeutic formulæ

recommended in the body of the book are conveniently brought together in the form of an appendix.

The two chapters on Diseases of the Internal Ear are among the best, and include an exhaustive study of *Tinnitus Aurium*, in its etiology, pathology, and treatment. The medicinal treatment of *tinnitus* receives special attention, and a most encouraging list of remedies is placed in the hands of the reader. But, alas, out of this large therapeutic armoury how few effective weapons are to be found! It is too true that this most frequent and distressing symptom generally resists each and all of them. Hydrobromic acid is, in the opinion of Dr. Jones, the most reliable; but in how few cases of *tinnitus*, not due to definite and curable pathological conditions in the external or middle ear, does this medicine exercise any good effect! In this relation we miss a reference to the interesting observations of Lucæ on the effects upon *Tinnitus Aurium* of holding close to the ear vibrating tuning forks of different pitch. Nevertheless, the description of the treatment of *tinnitus* is characterised by great discrimination as well as detail, and is worthy of study by those interested in this symptom—and what medical man as well as aural specialist is not?

The volume before us is really the third edition of the author's *Aural Surgery*, although the title is now changed and the matter very much amplified; indeed, if we mistake not, the work is practically altogether re-written. A number of coloured plates, depicting the more common morbid states of the tympanum, adds very much to the practical value of the book. The plates are taken from the author's *Atlas of Diseases of the Tympanum*, so widely and appreciatively known both in this country and in America.

We can cordially recommend this newest work of Dr. Macnaughton Jones. From its practical and reliable character it is very well fitted to impart to the practitioner a good knowledge of diseases of the ear, which, if combined with some clinical experience, will be of the greatest value in the course of practice.

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*Guide to the Administration of Anæsthetics.* By HENRY DAVIS, M.R.C.S.Eng. London: H. K. Lewis. 1887.

THE author rightly describes this in the preface as a "little work." It certainly is so in more senses than one. Most that is contained in it may be found in any surgical text-book which is at all up to date.

Taking into consideration the brevity of the work, the "Introduction" might very well have been left out, and the space occupied by it given up to more important matter. Among the "General Considerations" the statement is made that "the very best time to give an anæsthetic is in the early morning, when the stomach is quite empty." We very much doubt the accuracy of this statement, as we consider a patient much more invulnerable to shock during the course of a severe operation if he has partaken of nourishing and easily digested food three or four hours prior to the operation.

In Chap. I the author states that "the lint or inhaler should be sprinkled with a *small amount* of chloroform." And, farther on—"As soon as the agent begins to take effect, the patient, *especially* if he be addicted to alcoholism, begins to struggle." If the author will in future give the chloroform freely at the very first, without bringing the towel or inhaler suddenly down over the face, the word "*especially*" may be altogether erased, since he will then find that no others than drunkards will struggle.

The excitability of the conjunctival reflex is said to be "an *infallible* sign that the patient is not fully anæsthetic." This is certainly incorrect, as cases not unfrequently occur where reflex excitability is never quite absent, however deeply the patient may be anæsthetised. The condition of the pupil we hold to be of much greater importance, if thoroughly understood and carefully noted.

Undue encouragement seems to be given to the use of forceps in drawing forwards the tongue. If the elevation of the chin (to which the author rightly draws attention) be properly managed, the use of forceps will rarely, if ever, be found necessary.

Chap. II is much less open to objection than the preceding one. The statement, however, on page 28, that "in abdominal operations chloroform is preferred by the most experienced operators to ether" is surely wrong. Both Keith and Lawson Tait use ether. Again, that "ether is by far the safest anæsthetic agent we possess," we do not allow. Chloroform is quite as safe, *if properly administered*.

Chaps. III and IV are fairly accurate as far as they go.

In Chap. V it might have been stated that Faradisation is worse than useless. The hypodermic injection of ether, and the inspiration of anyl nitrite during artificial respiration, might have been mentioned with advantage.

Chap. VI calls for no comment.

*Manual of Bacteriology.* By EDGAR M. CROOKSHANK, M.B. (Lond.), F.R.M.S., Demonstrator of Physiology, King's College, London. Second Edition. London: H. K. Lewis. 1887.

It is only a few months back that we noticed the first edition of this work, and it is no little pleasure to us to be called upon so soon to draw attention to the second edition. It has been revised throughout and brought up to date, and in order to admit of a more concise arrangement of the species, the systematic part has been recast. Additional chapters have been written upon the general morphology and physiology of bacteria, upon antiseptics and disinfectants, and immunity.

Seventy-three illustrations have been added. An extensive list of references to works on bacteriology forms an appendix. It will thus be seen that the present edition is in many ways a considerable advance upon the first, and we wish it the same rapid circulation obtained by its predecessor.

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*A Treatise on the Theory and Practice of Medicine.* By JOHN SYER BRISTOWE, M.D., LL.D., F.R.S. Sixth Edition. London: Smith, Elder & Co. 1887.

THIS work has several times been very favourably noticed in our pages, and the frequency of new editions proves its acceptability with the profession at large. We have little to add to the commendations formerly expressed. The work is concise, practical, and thoroughly up to date. The changes in this edition denote the advance in medical science since 1885, when the last edition was issued. In the words of the preface, "The most important of these variations and additions are as follows:—An account of Pasteur's preventive treatment of hydrophobia; several emendations in the chapter on skin diseases; brief discussions on the subject of recurrent palpitation and Raynaud's disease; descriptions of actinomycosis and mycetoma; some modification in the chapter on sclerosis of the nerve centres; with the addition of articles on multiple neuritis, congenital tabes dorsalis and Thomsen's disease."

## REPORTS OF HOSPITAL AND PRIVATE PRACTICE.

CASE OF OPIUM POISONING. By J. K. LOVE, M.B., C.M.—About 2 P.M., on 25th June, I saw Mrs. B., a lady of 68 years, at her house in Crosshill. About 11-30 on that morning she bought an ounce of laudanum from a chemist in Crosshill, and just before my visit she was found sitting on a chair quite comatose, and with the empty bottle in her lap. Before I arrived she was stretched on the floor of the room. Her pulse was full, and about 50 per minute; respirations, 5 to 7 per minute. Pupils were contracted to a mere point, and her face was very livid. Artificial respiration was at once begun, and kept up for four or five hours without almost any intermission; cold affusions were applied to the chest, and the stomach was washed out with tepid water. About 6 P.M. Dr. Eben. Duncan saw the case with me, and we agreed to give subcutaneously  $\frac{1}{100}$ th of a grain of atropine. The respiration had risen to 13 or 14 per minute, and the pulse to nearly 100; and as the patient was much less livid, it was not thought necessary to continue the artificial respiration. At 7 P.M. the breathing was slower and more laboured, and the pulse nearly 120. We ordered very strong coffee to be prepared, and injected the solution into the stomach a little before 8 o'clock. The pulse continued to rise, the breathing to get slower and shallower, and the lividity to increase till death ensued shortly before 9 o'clock. Unfortunately a *post-mortem* examination could not be got.

*Remarks.*—Probably nearly all the laudanum was out of the stomach before the pump was used. Artificial respiration and cold affusions were followed by marked benefit, and in all cases of opium poisoning the former should be kept up as long as there is any hope of saving life. The atropia slightly dilated the pupils, but relieved none of the symptoms. Perhaps the coffee temporarily increased the rate of breathing. It should be given as early as possible if any stimulating or antidotal effects are counted on.

The patient was a dipsomaniac, and had attempted suicide by similar methods before. Indeed, there is some suspicion that she was in the habit of taking small quantities of laudanum.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY.

SESSION 1886-87.

MEETING IX.—11TH FEBRUARY, 1887.

*The President, PROFESSOR MACLEOD, vacated the chair to DR. JOSEPH COATS, Vice-President of the Pathological Section.*

## DISCUSSION ON THE PATHOLOGY AND TREATMENT OF CEREBRAL ABSCESS.

DR. WILLIAM MACEWEN, in opening the discussion, read a paper on the "Ætiology, Semeiology, and Morbid Anatomy of Cerebral Abscess," and afterwards gave, in brief outline, the cases which had come under his own observation.

*Mr. A. E. Barker, of London*, in the course of his remarks, pointed out that, in order intelligently to undertake the treatment of cerebral abscess, it was necessary scientifically to classify the different cases of abscess according to cause. He then enumerated the causes which most commonly give rise to abscess. With regard to diagnosis he thought that symptoms by which we could localise these abscesses did not help much, and that we should depend more on a careful study of their pathology. He then confined his remarks to the relation of cerebral abscess to ear disease. He thought that probably over 30 per cent of all abscesses of the brain were due to ear disease—possibly nearer 50 per cent. The application of antiseptic principles in the treatment of ear disease had not been sufficiently insisted upon. There were two ways in which cerebral abscess might form as the result of ear disease—(1) by septic phlebitis of meningeal veins; (2) by direct extension through the medium of a septic meningitis. He thought that during life we had to distinguish between five different very serious conditions depending on ear disease—(1) general septic meningitis; (2) plastic phlebitis of lateral sinus; (3) pyæmia, usually associated with diffuse phlebitis of lateral or petrosal sinuses; (4) abscess of cerebrum; (5) abscess of cerebellum. All these conditions might be grouped together in a single case. It was possible, in his opinion, to distinguish

them if one studied carefully the phenomena not only of cerebral abscess but also of the varieties of suppurative middle ear disease. He then sketched the treatment of different forms of suppuration in the middle ear in which constitutional symptoms of a grave type occurred, and where there might be a suspicion of intracranial disease. He next spoke of the signs and diagnosis of subdural abscess starting from the same cause, and was sure that the conditions mentioned were ten times more frequently met with than abscess of the encephalon. The main cause of abscess was chronic obstruction to the free outlet of pus. Again, abscess of the cerebrum was three times as frequent as that of the cerebellum. Nineteenths of the cases of cerebral abscess from disease of the temporal bone lie within a circle of  $\frac{3}{4}$  inch radius, with its centre  $1\frac{1}{4}$  inch above and the same distance behind the centre of the bony meatus of the ear. Most of this area of the brain is very tolerant of stimulation, and so there are no special nerve symptoms to aid in localisation. He then gave a sketch of the chief symptoms, pointing out that subnormal temperatures were most marked in the evening, and that cerebation was sluggish but perfect. He thought that we must be cautious in relying on optic neuritis as a necessary sign of cerebral or cerebellar abscess.

*Dr. Barr* limited his remarks to that frequent form of abscess in the brain which has its origin in a pre-existing purulent centre in the ear. The air-filled cavities of the middle-ear, especially the cavity of the tympanum, the antrum mastoideum, and the mastoid cells, are, in such cases, primarily and essentially the seats of the mischief. The disease in the ear is of the nature of a chronic purulent inflammation in the lining of these cavities, especially in the tympanum and antrum. The *acute* form of suppurative inflammation of the middle ear is very rarely, if ever, the cause of abscess in the brain. This predisposing cause of cerebral abscess is constantly with us in the persons of hundreds and thousands of the community. But it has been noted by various observers, including the speaker, that, in addition to this predisposing cause in the ear, there is also usually an exciting one, which provokes the mischief in the brain. An injury, such as a blow upon the head or ear, or an irritating influence acting upon the ear is frequently found to precede the pain in the head or the vomiting which ushers in the graver illness.

There is a general consensus of opinion that ear disease is distinctly the most frequent of all causes of cerebral abscess.

Judging from the numerous cases scattered through medical literature, and published within the past ten years—during which more close attention has been given to the ear in such cases—and also from the experience of our own hospitals, we are, I think, justified in attributing fully one-half of the cases of abscess in the brain to purulent disease of the ear. As time goes on, and as medical men come to use the ear speculum in every case presenting symptoms of cerebral disturbance, the frequency with which ear disease leads to a fatal issue will be found to be greater than has hitherto been suspected.

A very important question, especially as bearing upon the operation of trephining the skull, in such cases, is as to the most frequent situation of the abscess in the brain. In addition to his own cases, Dr. Barr—in order to gain fuller information, especially as to the position of the abscess—went over the published records of others in which the abscess in the brain was dependent on ear disease. The whole included 76, of which the abscess was in the cerebrum, generally described as the middle lobe, in 55 cases (or about 73 per cent of the whole); in the cerebellum, 13; in both cerebrum and cerebellum, 4; in the pons varolii, 2; and in the crus cerebelli, 1 case.

How is the disease in the middle ear propagated to the interior of the cranium? There are the two main directions by which this extension takes place; 1st, and most frequently, by the roof of the tympanum and antrum mastoideum to the middle fossa of the cranium and to the temporo-sphenoidal lobe of the cerebrum above. As shown by the published records of these cases, already mentioned, this is by far the most frequent path by which the disease invades the interior of the cranium. The second direction, in point of frequency and importance, is by the inner wall of the antrum and mastoid cells to the dura mater lining the posterior fossa and to the cerebellum, giving rise to a subdural collection of matter, or to a cerebellar abscess, or to both. In either or both of these situations, therefore, disease of the ear most frequently affects the interior of the cranium; and while carious apertures are by no means always present there, they are frequently found.

With respect to treatment, the recent almost unique cases of Mr. Barker, Prof. Greenfield, and Dr. Macewen have inaugurated a new era in the previously hopeless—dismally hopeless—cases of cerebral abscess. Previous to the cases of these gentlemen, Dr. Barr knew of only two recorded instances of recovery after trephining the skull for cerebral abscess, both due to ear disease. One by C. Tauckenbrod, Hamburg, reported



in the *Archives of Otolaryngology*, vol. xii, Nos. 2 and 3. The other case was by Dr. Schondorffs, reported in the *Monatschrift für Ohrenheilkunde*, 1885, No. 2. Here there was, however, a fistular orifice in the skull leading to the brain, where the abscess existed. The method pursued by Dr. Macewen seems to be the most reasonable—namely, to open the skull about two inches above the ear, and, when the abscess is found, to make another opening lower down, nearly on a level with the floor of the middle fossa of the skull. If a current of fluid can be made to pass from the upper to the lower opening, thorough cleansing would be ensured. Dr. Barr was not sure of the desirability of invariably opening into the mastoid cells as preliminary to the larger operation. If a case presented indisputable signs of intra-cranial mischief, and if there was neither redness, swelling, nor tenderness over the mastoid process, he questioned the propriety of opening into the latter. But when every member of our profession is sufficiently impressed with the importance of chronic suppurative inflammation of the middle ear, and prepared to efficiently treat this disease in all its stages, the occasion for this operation would probably seldom arise.

Dr. W. F. Somerville stated that he had recently, at Dr. Barr's request, analysed the urine in two cases of cerebral abscess, according to the method originated by Haswell of Vienna. He found that when pus was present in the brain, the urine was conspicuously abnormal. There were marked evidences of the occurrence of febrile processes in the body, though at the time little or no rise of temperature was indicated by the thermometer. The chlorides were far below the normal, signifying that effusion was taking place; whilst the phosphates were much increased in quantity. This increase, moreover, included an increase in the elimination of the earthy phosphates, which, together with other facts, suggested excessive metamorphosis of the cerebral nervous tissue. One of the urines so examined was from a case successfully operated upon by Dr. Macewen, so that an opportunity arose of contrasting the urines before and after operation. As soon as the pus was removed, the urine showed a change in all the above features, till on the 13th day after operation its characters were normal. These changes in the urine advanced *pari passu* with the improvement in the patient's general condition.

## 12TH FEBRUARY, 1887—ADJOURNED MEETING.

*Professor Greenfield, of Edinburgh*, said : \* —I feel, Sir, that you have placed me in a somewhat difficult position in asking me to resume the discussion to-day. For what I had to say of a constructive nature has been almost entirely anticipated, and far more ably stated, in the admirable papers of Dr. Macewen, Mr. Barker, and Dr. Barr. I shall best serve the purposes of this debate if I attempt to bring forward all the possible objections that I can find to urge, and to raise some questions upon which more definite information appears desirable.

I should like, in the first instance, to state my full concurrence in much that has been said ; especially in the admirable description of the symptoms by Dr. Macewen, and in all, or nearly all, that was said by Dr. Barr. But my immediate business is to find and state points of divergence.

When one looks into the subject of cerebral abscess, one is immediately struck with the comparative paucity of the literature. Every one cites the same statistics, and the most reliable are now of old date. Other facts concur in making it probable that abscess of the brain is comparatively rare. Thus, the statistics of St. Thomas' Hospital, taken at random from a consecutive series of 13 years, show only 15 fatal cases of abscess, 8 cerebral, 4 cerebellar, 3 position not stated. During the same period there were 6 fatal cases (of which 4 were confirmed *post-mortem*) of acute meningitis following ear disease, in 1 with thrombosis of the lateral sinus.

Inquiries amongst friends, and my own experience, equally confirm the rarity. Thus, in fourteen years, I find exact records of only 11 cases (including 1 of meningitis associated with ear disease), either under my own care, or in which I have had opportunities of exact observation during life and after death. Of course I have seen other cases, but have no sufficient record of them. Of these cases the cause was—ear disease, 7 ; injury to skull, 1 ; pyæmia, 1 ; old suppurating cavity in lung, 1 ; no discoverable cause, 1.

In cases of abscess, the abscess was situated in the temporo-sphenoidal lobe in six, all associated with ear disease. In two of these it was the size of a walnut, and in close contiguity to the roof of the tympanum, one actually communicating. In one case it involved the occipital lobe as well as the temporo-

\* Only an abstract, containing the more important points, as nearly as possible in the words used, is here given.

sphenoidal, and was associated with thrombosis of the lateral sinus. In two it extended towards the front of the lobe.

Of the other cases the abscess was in the frontal lobe in one, cause not discovered; in the upper part of the hemisphere, near the middle, in one (pyæmic); in one it extended widely in the upper part of the hemisphere, near the surface. The latter was of especial interest, as it followed a contused wound without fracture over the right frontal bone, with consequent necrosis, but the abscess and meningitis were entirely situated on the left side, and were apparently secondary to a septic phlebitis of the longitudinal sinus.

The other points in these cases confirm fully the statements of Dr. Barr, and agree with the views expressed by Dr. Macewen of the septic causation of cerebral abscess.

From a comparison of the records which I have been able to collect, I should demur to the statement of Mr. Barker that such results as meningitis, thrombosis, pyæmia, &c., are ten times more frequent than cerebral abscess as results of ear disease. I should, indeed, be inclined to reverse the proportion, although that would also be inaccurate.

In what I have to say further, I shall limit my remarks almost entirely to abscess secondary to ear disease, which comprises nearly all the cases falling under the care of the physician.

First, as to the pathology. Dr. Barr stated that abscess did not occur as a result of acute inflammation of the ear. Although in the vast majority of cases the ear disease is chronic, I do not think that we must assume that it is always so. One case which I saw several years ago was that of a child suffering from rickets, with no known ear disease. This child took scarlet fever very severely, with great swelling about the glands of the neck, followed by a discharge from both ears. On the seventeenth day repeated convulsions occurred, followed by rise of temperature, and death in three days, with temperature of 109°. *Post-mortem*.—The dura over the roof of the tympanum was bathed in pus, and acutely inflamed, and immediately over it an abscess cavity, the size of a walnut, was found in the brain substance. The tympanic cavity showed inflammation, apparently recent; the layer of bone forming its roof was discoloured.

Although it is possible that there was previous ear disease, all the facts of the case appeared to indicate that the disease followed the scarlet fever, and constitute a sufficient ground for suspending judgment upon this question.

I may next say a few words as to the pathology of cerebral

abscess. There can be little doubt that in the majority of cases the condition is due to septic absorption, usually accompanied by septic phlebitis. Commonly the dura is first involved in inflammation, then the brain becomes adherent, though this is not always the case. The abscess, when chronic, is usually enclosed in a thick capsule of vascular granulation tissue; the inner surface of this is granulating and eroded, and the contents always foetid. It seems to me very probable that the initial condition is a local gangrene of the brain tissue, spreading from the vein or veins first affected, and the wall of the abscess is a limiting membrane formed by the irritation and proliferation of the connective tissue around. Once formed, the membrane serves to separate the decomposing matter from the surrounding brain tissue. The abscess may slowly enlarge by suppuration from the interior of the sac. But this enlargement is not necessarily or even usually accompanied by any further acute inflammation of the brain, until from some additional exciting cause the inflammation may spread, and acute softening or meningitis lead rapidly to a fatal issue.

The view of the pathology of cerebral abscess thus briefly sketched has an important bearing on the course and treatment of the disease. For the abscess may exist for a very long period without any symptoms, and we must not expect to observe necessarily, for example, any rise of temperature during its existence. It seems probable that the cases referred to by Dr. Barr of abscess occurring as the result of a blow, &c., are to be accounted for by the lighting up of fresh inflammation in and around an old abscess.

And even when the abscess begins to enlarge, the symptoms may be, and for the most part are, those simply of compression, closely paralleled by some cases of slowly occurring meningeal hæmorrhages, and the rare case of chronic non-tubercular hydrocephalus in the adult, or of hydatid cysts in the brain. The occurrence of rigors, of irregular temperature, or of acute cerebral symptoms, more commonly indicate meningitis, increased inflammation of the ear itself, or the occurrence of inflammatory softening around the abscess.

What, then, are the diagnostic symptoms of the presence of abscess? Evidently those cases in which the symptoms are least urgent and striking are those in which we may expect the best results from treatment, if we can make the diagnosis during the latent stage or early in the course of the more pronounced symptoms.

Of those general symptoms which may aid the diagnosis

from those diseases likely to be mistaken for it, *rapid wasting* is very important. This has been very marked in some of my most typical cases.

As to *temperature*, I have said that it may not be elevated during the time that the patient is under observation, and in some cases, notably in one of trephining which I have recently published,\* it was subnormal throughout. It was suggested by Mr. Barker that this subnormal temperature was due to cerebral inhibition. But without stopping to discuss the question, I may suggest another possible cause—viz., the absorption of some of the products of septic decomposition from the abscess cavity. A parallel observation has been made by Dr. Thomas Keith upon some cases of suppurating ovarian tumours with low temperature.

Next, with regard to *optic neuritis*. It is too generally assumed that optic neuritis is constant in cerebral abscess. Here I must absolutely join issue with my friend Dr. Gowers, whose views Mr. Barker seemed to endorse. Briefly stated, the facts at present recorded are, I believe, insufficient to enable us to draw any decisive inference either from the absence or presence of optic neuritis. It was absent in two of my cases which were carefully examined from time to time during life and *post-mortem*, and in one case was unilateral, and appeared to commence only during the period of more urgent symptoms. Hence I can state absolutely that it is not constant. Moreover, there is no doubt that it may be present in cases of meningitis without abscess. It will be very desirable, in order to settle this question, that some aural surgeon should examine the eyes of a long series of cases of chronic suppurative otitis, in order to determine what conditions are present.

Next, as to *localising* symptoms. These are usually absent, and when present, may be misleading. Thus the headache may be frontal when the abscess is in the cerebellum. Localised headache or tenderness on percussion may, however, be present. In the recent case of temporo-sphenoidal abscess already cited the headache was frontal, but the patient has stated since recovery that he had much more pain on the left than the right side. In that case also localising symptoms due to nerve pressure were present in a rare combination and degree.

It is sometimes suggested that the presence of tenderness and perhaps swelling in the mastoid region may indicate especial involvement of the mastoid antrum and cells, and

\* *Brit. Med. Journal*, 12th February, 1887.

consequent probability of extension of disease to the wall of the lateral sinus. Curiously enough, in those cases which I have seen where this involvement occurred, there was neither tenderness nor œdema.

It may be concluded from these observations that I regard the diagnosis of cerebral abscess, when latent and in the best position for cure—viz., in the temporo-sphenoidal lobe, as at present difficult; and if this discussion should have, as its sole result, a greater precision in diagnosis at an early period, it will be a result of the greatest value.

Upon the subject of operation I speak with diffidence, desiring rather to ask some questions of the surgeons present. Of one thing I am sure, that the operation should not be delayed for an hour when the symptoms are decided, and especially if coma is supervening. In my own case a delay of two days occurred from unavoidable causes, but I trembled for the result.

As to the preliminary measures recommended by Mr. Barker, they may no doubt be of value in some cases, or as an additional method, but if, as in many cases, the condition is one where there is grave risk in delay, should not the abscess be opened at once? If the lateral sinus is involved, ought it to be meddled with? I think the risk a great one.

But the cases most likely to succeed are those of encapsuled temporo-sphenoidal abscess. Where should the trephine be applied? The point suggested by Mr. Barker in the successful case published by Dr. Gowers and himself is at its centre, from  $2\frac{1}{2}$  to 3 inches from the centre of the point at which Mr. Caird trephined in my own successful case. I must confess I am unable to see any ground for choosing a point so far removed from the usual site of the abscess. From a comparison of the records of cases and examination of crania, I believe that the proper region is that selected by Dr. Macewen for his lower opening—viz., at a point near the junction of the squamous and petrous portions of the temporal, and nearly opposite the roof of the tympanum.

Ought there to be two openings? If the abscess communicates with the tympanum, ought the opening to be enlarged if possible and the communication maintained? May it not yet be possible to reach the abscess through the roof of the tympanum, in spite of the failure of former attempts? And in such cases, where there is chronic disease of the tympanum, should not the perforation of the membrana tympani be enlarged by incision, and the cavity freely opened in order to ensure thorough antiseptic treatment? These and other

practical questions appear to me to merit the careful attention of surgeons. I feel sure that at no distant period we shall be able to recommend trephining as an exploratory and diagnostic measure in doubtful cases.

*Mr. F. M. Caird, of Edinburgh*, briefly referred to Professor Greenfield's case, in which he had performed the operation necessary for the evacuation of the abscess.

*Dr. Alexander Robertson* said—It is with some hesitation that I take part in this discussion. The reason is that although I have met with a considerable number of cases of abscess of the brain, all of which I have records, with one exception, occurred a number of years ago, for the most part before the era of localisation of function and exact description of the position of lesions. My remarks will therefore be brief. Two of my patients suffered from constitutional syphilis, but the conditions were very different in the respective cases. In the first one there was necrosed bone. Under it there was a layer of pus. In the substance of the hemisphere corresponding with this purulent layer there was an abscess of the brain, purulent matter being intermixed with brain substance. It was circumscribed, but no distinct cyst wall existed. The sequence of events in this case was very clear, and yet there was no evidence of continuous extension from the bone inwards, for the surface of the brain was not involved. There may have been extension of inflammatory action into the interior along the walls of the blood-vessels, as happens sometimes in abscess of the temporo-sphenoidal lobe, when the superficial layer over it is not implicated. In the other case there was a well defined abscess containing about 2 oz. of yellow pus. A distinct vascular lining membrane of the cavity existed. The brain in its neighbourhood was softened and paler than usual. There was no history of injury to the head, nor was there a focus of suppuration observed anywhere. If there was none, then syphilis alone apparently gave rise to the abscess, apart from any ordinary septic cause. In one case a large, well defined, encysted abscess existed in the substance of the left anterior lobe, with a small opening into the lateral ventricle of the same side. The only cause that could be assigned for its occurrence was the existence of suppurating cavities in one of patient's lungs. Purulent matter had obtained access into the circulation. There does not appear to have been general pyæmia, and the abscess in the brain was single. In one case there was a large abscess, containing 9 oz. of healthy pus, underneath the superior, middle, and inferior temporo-

sphenoidal convolutions. There was a bit of necrosed bone in front of right ear. Although the abscess in the substance of the brain was situated close to this bit of dead bone, there was no direct communication between them, apparently healthy tissue intervening.

Convulsions occurred in most of my cases; yet it is to be observed that they were present only when the motor region of the brain, either superficial or deep, was encroached on, or was near the wall of the abscess. Thus, in the abscess of the temporo-sphenoidal lobe there were no convulsions, and they were also absent in another case, where it was situated in the posterior lobe. In one patient, a woman, age 21, after a group of severe convulsive fits, and a second group on the second day afterwards, there was an interval of three weeks in which she felt so well, though not altogether free from headache, that she insisted on walking about the ward and attending to her child. At the close of that period, however, one day she fell suddenly on the floor, and died in a few minutes. The abscess was an encysted one, and had not burst. It was not clear what was the cause of the suddenness of the death. It is to be observed that the situation of the pain in the head complained of by the patient is not a reliable indication of the position of the abscess. Thus, severe frontal headache was complained of by the patient, who had the abscess in the temporo-sphenoidal lobe. Percussion of the skull may, however, as was observed by Dr. Macewen in more than one of his cases, elicit pain over the seat of the abscess. This mode of investigation should, I think, be regularly practised as a means of diagnosis in all cases where there is any suspicion of localised disease on the upper or lateral surfaces of the brain, or in the membranes covering these parts. The nearer the lesion approaches the surface, and more particularly when the membranes are involved, the more likely is percussion to prove useful. Further, the skull generally should be percussed in all cases, and one part compared with another, otherwise erroneous conclusions are likely to be arrived at. The class of cases in which the greatest practical good is likely in future to follow operation, is just the one which has already yielded the successful cases in the practice of the gentlemen who have spoken in the course of this discussion—namely, abscess resulting from disease of the middle ear. When abscess does form from that cause, and is not of a pyæmic kind, it is almost always in one of two situations—namely, the temporo-sphenoidal lobe and the cerebellum.

Other speakers have already pointed out the differential



symptoms in such cases. When the abscess is elsewhere, the symptoms are probably most localising when it is situated in or in the vicinity of Broca's lobe, and implicates speech. The more restricted the convulsive movements or the paralytic weakness, the more exact is the indication of the site of the disease; *e.g.*, a strict limitation of the convulsive action to one arm, or no defect save that of speech. It was this very definite limitation which led Dr. Macewen, eleven years ago, to propose cutting down on an abscess in his patient's brain, and which he would have done had his colleagues consented.

In considering the propriety of operation, it is well to bear in mind, in connection with such abscesses as are not dependent on disease of the ear, or do not result from injury, that there is the risk of the abscess being multiple, and that though we may correctly diagnose the seat of the superficial one, should the deeper one or ones be so placed that they do not irritate the motor region, there may be no additional symptoms. In such case the surgeon will be unable to save the patient by operation. Multiple abscesses are most apt to occur in septicæmic conditions, or in connection with centres of suppuration in some other organ, but even when of that origin the cerebral abscess may be single, as one of my cases showed.

*Dr. Workman* said, after what has already been said on this subject, I shall only say a very few words. There is a point I wish to refer to that Dr. Macewen treated at great length, so that I fear he only obscured where he intended to throw light. It is the fact that abscess in the brain never occurs without some septic germ having previously gained admission. I think it will simplify this when I point out what is now, I think, almost universally admitted, that the formation of pus never takes place without the presence of micrococci.

Even in cases of tubercle or anthrax you will not have pus formed unless micrococci are also present at the point where it is forming.

This is not absolutely true, for, as my friend, Dr. Passet, of Munich, has shown, some chemical irritants will cause the production of pus without bacteria being present. And the pus so produced contains no bacteria and will not affect other animals which may be inoculated with it.\* This is beside our subject, and I shall not discuss it farther, but if any of

\* This is only possible by the most careful precautions, and therefore is only an instance of the exception which proves the rule. The chemical irritants used by Passet were croton oil and turpentine.

you want to study it you will find more about it in *Untersuchungen über die Ätiologie der eitrigen Phlegmonie des Menschen*, by Dr. Joseph Passet.

I now wish to say a few words as to disease of the middle ear.

I am convinced that when a case of middle ear disease comes before us with any symptom of meningeal or brain affection, we should at once advise the patient to have the mastoid laid open to allow the middle ear to be treated in a thoroughly aseptic manner. If this treatment were systematically carried out I feel convinced that abscess of brain or meninges would much more seldom take place.

This should, I think, be done even although the disease may have a tubercular origin, and without regard to the healthy appearance of the mastoid from the outside, as it gives great relief to the patient and may prolong life considerably.

Four years ago I had a case of disease of the middle ear in Belfast. While we (an aural surgeon and myself) were treating the middle ear, the patient had a rigor and a convulsion, chiefly confined to the face, ending in coma. The left ear was the one affected. The surgeon then opened the mastoid while the boy was in this comatose state. Nothing was found, but the hæmorrhage appeared to act like a charm, and the boy became conscious.

After this a curious symptom appeared. The boy, 18 years old, got into a state resembling aphasia. He appeared to have forgotten a lot of words, &c. For example, one was how to indicate Dr. — (the surgeon). And when he wished to know if Dr. — were coming he would say, "Is the man coming?" Also, he was quite unable to say "Dr. —" after any one. His condition resembled what Munk calls "Seelentaubheit." The boy again had rigors, and we decided, largely influenced by this peculiar symptom, to open the skull over the temporo-sphenoidal lobe and seek for the abscess, which we suspected was either in that lobe or pressing upon it. Dr. — decided to use a gouge, and with it he made an opening about 1½ inches above the meatus.

Whenever the gouge passed the inner table it slipped into a large abscess from which the pus welled up, about an egg cupful escaping. A night or two after the operation the boy had another convulsion, and for it was blistered on the back of the neck. From that time he steadily recovered. All the symptoms passed rapidly away except the otorrhœa, which still persists in a slight degree.

I believe in this case we had to do with a subdural abscess,

as, after the operation, a soft probe was used, and it seemed to glide easily backwards, downwards, and inwards along the petrous bone. A drainage tube was inserted and the boy directed to lie on the affected side, in order to drain the cavity as perfectly as possible.

I think that the peculiar aphasic condition may be of use in diagnosis, if it is found to exist in other cases of a like sort. It did not resemble a case of aphasia from disease of Broca's convolution.

*Dr. James Erskine* had noted that in some cases of cerebral abscess the tympanic membrane was described as containing a comparatively small perforation. In order, therefore, to secure free drainage, and prevent the dangerous and fatal complications that may attend chronic purulent otitis media, he advocated the removal of a part or the whole of the membrana tympani in cases of that disease which resisted ordinary treatment. In chronic suppuration, with a perforation in the membrana Shrapnelli—which is most obstinate to treat, owing to the difficulty of drainage from the situation of the disease—he thought the best result would be obtained by extirpation of the malleus, as proposed and carried out by German aural surgeons. Opening into the mastoid antrum might be resorted to more frequently, and at an earlier period, than practised at present, for the purpose of producing a cure of chronic purulent inflammation of the middle ear not amenable to treatment by other means.

*Dr. Erskine* thought it was to be regretted that the condition of the urine rather than of the ear had been so carefully described by *Dr. Somerville* in the case of cerebral abscess arising from purulent otitis media to which reference had been made in the course of the discussion.

With regard to the proportionate frequency of cerebral abscess arising from ear disease as compared with other causes, considerable disagreement existed amongst observers. For example, *Libert*, in the year 1856, gave the proportion as 1 in 4; and *R. Meyer*, in the year 1867, stated it, according to his observations, as 1 in 2. Probably, as more exact diagnosis is made of all cases of cerebral abscess, it will be found that the majority of such cases arises from purulent disease of the middle ear. So far *Dr. Erskine's* observations had been confined entirely to cases of ear disease, and he roughly estimated that cerebral abscess occurred once in about 800 cases of affections of that organ presenting themselves for treatment.

*Dr. Joseph Coats* said that, in looking over his records of *post-mortem* examinations in the Western Infirmary, he found

that there were 9 cases of abscess of the brain, or, including 1 of leptomeningitis from ear disease, 10 cases. Of these, 5 were connected with ear disease and 5 were not. In 2 of the latter cases the abscesses were due to injuries to the skull, while in the remaining 3 the cause was obscure. The ear was examined in these latter cases, but no disease was found. Of the cases due to ear disease, 2 of the abscesses were in the temporo-sphenoidal lobe of the cerebrum and 2 in the cerebellum. In the case of leptomeningitis the inflammation extended to the soft membranes generally and to those of the cord.

Proceeding to comment on the pathology of cerebral abscess, Dr. Coats referred to specimens which he had placed under the microscopes, and more especially the preparations from a recent case of Professor Gairdner, which he had examined with some care. Continuing, he said—

One striking fact in connection with abscesses connected with ear disease is that they are, probably in every case, foetid abscesses, the foetor being of no ordinary kind, but suggesting a gangrenous condition. In the four cases examined at the Western Infirmary this foetor was very striking, and was observed before laying open the brain, the mere exposure of its surface causing a strong odour to exhale. This is the more striking as the other five abscesses were odourless. This fact, which I believe to be a characteristic of cases of abscesses connected with ear disease, puts them out of the category of ordinary acute abscesses, which as a rule have no odour. In most acute inflammations, as Alexander Ogston was the first to demonstrate, there are present, in the pus and in the wall of the inflamed cavity, micro-organisms, having the globular form which distinguishes them as micrococci. There are several different kinds of these, some of which, when cultivated, grow in chains (the streptococci) and some in masses (the staphylococci). These micro-organisms abound in most acute septic inflammations, in erysipelas, in acute peritonitis, in abscesses forming in the loose connective tissue near septic wounds. We may infer that these various forms of micrococci alone among the many micro-organisms in putrid fluids have the power of multiplying and extending in the living tissues, where, by their products they produce acute suppurative inflammation. Now, in connection with the present subject it is important to note that the products of these micrococci are apparently odourless, or at least free from putrid odour. Acute abscesses are usually devoid of the odour of putrescence, and so is the fluid

in acute peritonitis if obtained fresh, before foetor can be communicated through the intestinal wall. But the abscess in the brain from ear disease has a strikingly foetid odour, and in connection with this it is noteworthy that, instead of one micro-organism we have here many different forms. I have placed under the microscope a preparation made from a drop of pus from such an abscess, stained by Gram's method, so as to show the micro-organisms present. In this preparation there are large and small cocci, some of them in chains, there are short rods or ordinary bacteria, and long rods or bacilli; some thick and more deeply stained; others thin and more faintly coloured; some growing into threads and others not so elongated. I should say that, judging by the form and size alone, there are not less than 12 different kinds here, and there are probably many more. It is not an ordinary septic abscess, but rather a putrid or gangrenous cavity.

Perhaps we should associate with this the fact that the lining of the abscess does not show colonies of micro-organisms as that of an acute abscess does. I have carefully stained by Gram's method many sections from this case, and there are only a few isolated micro-organisms to be found, evidently only such as have been deposited from the contents. The abscess has a distinct lining membrane formed by a layer of granulation tissue, the granulation cells apparently forming a barrier which prevents the penetration of micro-organisms. This fact is one of considerable interest in its general relations to surgery; the active cells of a granulating wound prevent the penetration of micro-organisms, so that in this respect a fresh wound is much more dangerous than a granulating one. But although the penetration of micro-organisms has been prevented, the inflammation has extended somewhat beyond the wall of the abscess. In two of the sections under the microscopes it is seen that the connective tissue at a distance from the interior of the abscess is much infiltrated with round cells and the blood-vessels are greatly distended. In this inflamed tissue, however, there are no micro-organisms; it is merely the dissolved products of putrefaction which have been carried beyond the cavity.

I am inclined to make a good deal of this putrid character of the abscess, in relation to the pathology of the condition. It means surely that the lesion must have had its origin in a very putrid condition of the discharges in the middle ear. I presume that the ordinary condition of otorrhoea implies that the membrana tympani is perforated—destroyed to a greater or less extent. The cavity of the tympanum is in the unusual

condition of exposure to the external air. Owing to this exposure it is liable to catarrhs just as the nares and the pharynx are, and we have recurring attacks accompanied by discharge of pus and mucus. Every practitioner must be acquainted with persons who are periodically subject to such attacks. These would not have much importance, any more than catarrhs of the nares, were it not that the discharges lying in the tympanic cavity and in the complicated mastoid cells, being exposed to the air and stagnating in the recesses of the cavities, are peculiarly apt to undergo putrid decomposition—the discharges are proverbially ill smelling. When once putrescence has occurred the products of decomposition are themselves sufficient to keep up the inflammation, and so the discharge is apt to become permanent. We have as the result of the prolonged inflammation those thickenings and adhesions and polypi of which we hear so much, these all consisting of new-formed granulation tissue or connective tissue which has been produced as a result of the inflammation. These acrid discharges, should anything occur to hinder their free exit, are apt to do further damage. They may cause ulceration of the mucous membrane, and that destruction of the bone which has been so well described by Dr. Barr.

The extension from the ear to the interior of the skull can generally be traced without much difficulty, and it is usually a putrid gangrenous process. In the case to which I have made special reference the bone has been eaten into, and a perforation of the roof of the tympanic cavity has occurred. The dura mater was adherent, and it could be seen that it was partly sloughing and infiltrated with pus. The brain was adherent, and the abscess cavity was close to the surface at this point. The inflammation had also extended along the subarachnoid space, as we found signs of acute inflammation in the fissure of Sylvius, over the inferior surface of the cerebellum, and over the base of the brain as far forward as the optic chiasma. I apprehend that in the extension to the brain there is a direct advance of the putrescence and necrosis—what I may call a brutal advance as compared with the more stealthy encroachment of the growing micrococci.

I should like to call attention here to a point which may be of some importance in relation to those rather striking operations on the brain which have recently attracted so much attention. The surface of the brain is covered by the arachnoid membrane, which is usually described as a serous membrane. The internal surface of the dura mater is in contact with this membrane, and these together enclose a cavity which used to

be called a serous cavity, but which is not now generally regarded as such. Whether this be a serous cavity or not, it has always appeared to me to be a remarkable fact that inflammation scarcely ever shows itself on the outer surface of the arachnoid. We frequently meet with subarachnoid inflammations, but no fibrine or pus appears on the outer surface of the membrane. In a case of ear disease we may have putrescence extending across this space from the dura mater to the brain, and it may spread along the subarachnoid space, but the outer surface of the arachnoid and the internal surface of the dura mater remain unaffected. One can hardly suppose that in travelling across this space there were not some of the micro-organisms of decomposition conveyed into the space. The contrast to this is very great when we consider the condition of the pleura or peritoneum under similar circumstances. The passage of the most minute portion of putrid fluid into one of these cavities causes an acute inflammation, associated with the multiplication of micrococci. These remarks apply not only to the case of extension from disease of the ear, but to suppurative inflammations of the membranes of the brain in general. It is very seldom indeed that you find pus on the internal surface of the dura mater or the external surface of the arachnoid. If this be true then it diminishes the risks of injuries or operations which expose the surface of the brain. The risk of opening into this space is not comparable to that of opening the pleura or peritoneum.

After Dr. Macewen had briefly spoken,

*Mr. Barker*, in reply, said that he had to express his thanks for the criticism which had been made on his paper, which indeed was written with a view to elicit criticism. And Professor Greenfield's remarks, far from being too critical, perhaps erred on the side of mildness. In regard to what the Professor said about the paucity of material, he was entirely in accord with him. As yet they were only feeling their way on the subject, and they could only suggest what they conceived to be safe conclusions. In regard to what he had stated as to the comparative numerical proportion of two classes of cases of brain abscess, he would read again the sentence criticised. [*Mr. Barker* read the passage, and Professor Greenfield said that he had not misunderstood it.] To that statement he was prepared to adhere, and he believed that the statistics which he could adduce would prove it; but he would be obliged to include living cases in the larger group. As to the case which Professor Greenfield described of what he believed to be

subdural abscess, the result of acute disease, was he prepared to say that before the scarlatinal attack there was not some discharge from the ear, some suppurative affection of the middle ear which had been overlooked? Such cases were not uncommon, and he would suggest that what really happened was that the scarlatina merely set up mischief, the germs of which were there before. Therefore he would not take that case as proving anything against his proposition. Dr. Greenfield was also somewhat inconsistent in taking exception to his proposal to proceed by way of exploration, and in a subsequent part of his address he went on to encourage exploratory procedure to an extent which he (Mr. Barker) had never ventured on. In regard to operating in disease of the middle ear, he would have no hesitation, and looked upon it as almost minor surgery. He still thought it would be desirable to operate in the neighbourhood of the inflammatory focus, in order to prevent the severe operation of opening the skull. The operation was not formidable. Every bit of the mastoid process could safely be taken away, and in respect of the mastoid bone he would have less respect for it. The lateral sinus could be treated with far less respect, and the covering of the sinus could be explored without danger. Had he a child of his own so affected, he would feel far easier if the bone were away than if it remained a constant source of danger. He had acted on that principle in private practice, and had seen no reason to regret it. In regard to optic neuritis, he quite concurred with the view of Dr. Greenfield, and had, in fact, enunciated the view in his paper. He felt satisfied that there were several conditions of the middle ear which might produce optic neuritis independently of abscess. They must be extremely cautious in drawing conclusions here; and so much impressed was he with this that he had taken steps in the direction of having every case of suppuration of the middle ear examined and reported on, as to the condition of the optic disc. He examined the cases himself, and in any doubtful case he called in the aid of the ophthalmic surgeon. Already he had accumulated a great many notes, with indications of some curious results. In regard to the spot which he had indicated for operation, it was that at which the operator would be most likely to strike pus. [Mr. Barker proceeded to illustrate this by reference to a diagram.] He concluded by again thanking the Society for the opportunity they had afforded him in taking part in the discussion.



## PATHOLOGICAL AND CLINICAL SOCIETY.

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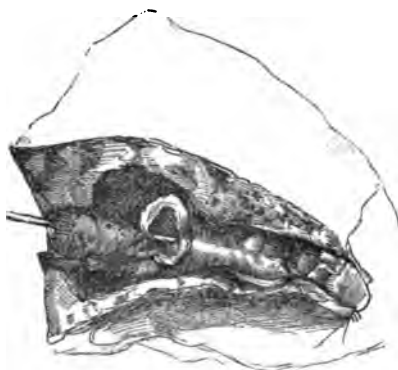
*The President, DR. JAMES FINLAYSON, in the Chair.*I.—ANEURISM IN A PULMONARY CAVITY: DEATH FROM  
HÆMORRHAGE.

BY DR. JOSEPH COATS.

IN this case blood was found in great abundance in the air-passages, and it was known that large quantities had been expectorated during life. Some difficulty was experienced in discovering the source of the hæmorrhage. The general condition of the lungs is thus described. "The left lung is firmly adherent posteriorly and there is considerable emphysema of the base and lower edge of upper lobe. The apex is occupied by a cavity measuring an inch and three-quarters in diameter, and with a comparatively smooth internal wall, which is lined with a distinct membrane. Elsewhere, both in the upper and lower lobes, there are numerous scattered caseating condensations and several small cavities. The pleura is greatly thickened, the two adherent layers measuring nearly half an inch in thickness. The right lung is also extensively adherent, and the upper lobe of this lung is considerably contracted, the apical parts being occupied by pigmented cicatricial tissue, with comparatively little appearance of tuberculosis. There are a few small smooth-walled cavities. The lower lobe also shows cicatrices and some bronchiectasis."

The source of the hæmorrhage was evidently to be looked for in the left lung, and on examination it was found that there was a great accumulation of blood in a bronchus passing towards the anterior margin of this lung about two inches down from the apex. This bronchus led into a small cavity about three-quarters of an inch in diameter which was completely filled with somewhat dense coagulum. On removing this, the aneurism shown to the Society was displayed. It is about the size of a pea, and it is seen to be completely torn open, the edges of the tear being everted. The aneurism forms a dilatation of a branch of the pulmonary artery, and a probe has been passed into the latter, through the aneurism

and on into the artery beyond. It can be seen in the bottom of the aneurism. The cavity in whose wall the aneurism was found is obviously formed by a saccular dilatation of a bronchial tube, and the bronchus is continued beyond the cavity although still somewhat dilated. The artery is perfectly pervious both on the proximal and distal sides of the aneurism. Beyond the aneurism it lies just outside the dilated bronchus, whose wall is greatly thinned, and the probe shines through the wall of artery and bronchus as if there was very little tissue between. One point of importance in connection with the clinical history is that the blood in the cavity was generally dense, but it was specially so around the



ANEURISM IN A PULMONARY CAVITY.

The cavity is a saccular dilatation of a bronchus, which continues dilated beyond the cavity. A probe has been introduced into the artery and passed through the aneurism into the distal part of the artery.

aneurism forming a kind of mould of the aneurism, the internal surface of the mould being smooth and like a membrane.

The patient was twice in the Western Infirmary under Dr. Gairdner, from whose ward journal the following history is taken. The history is that of a somewhat chronic case of phthisis which at the time of death had lasted nearly two years, and was cut short by the hæmorrhage while the disease was not very advanced. The temperatures were slightly febrile, the highest being  $101.8^{\circ}$ . He had been progressing fairly well when, on the evening of the 24th November, 1886, he had an attack of profuse hæmoptysis, coughing up about 7 ounces of florid red blood. He was kept at perfect rest, ice poultices were applied to the chest and ice was given to suck.

About 4 A.M. on the 25th there was another attack of hæmoptysis, when about 8 ounces of blood were expectorated. The patient was found lying in a state of collapse, the fluids rattling up and down in the bronchi, the breathing laboured and the pulse rapid. Some brandy was administered and the patient rallied somewhat and was able to speak. A drachm of liquid extract of ergot had been previously given, ice was now applied to the chest and hot water bottles to the feet. The patient now revived greatly and continued on all day, but in the evening he had a third attack of hæmoptysis, losing 5 or 6 ounces of blood. Next day (the 26th) he was in a very weak condition, and in the evening, about 7 P.M., he had another attack, in which 4 or 5 ounces of blood were lost. He was quite conscious all the time and lay flat on his back breathing heavily. He remained in this condition, gradually getting weaker, and complaining of great pain low down in the left side, where a râle which was interpreted as pleuritic friction was detected. He died about 3 in the morning of the 27th.

In commenting on the case, Dr. Coats pointed out that in cases of phthisis with cavity such as this, a sudden large hæmoptysis was in the great majority of cases due to the rupture of an aneurism in a pulmonary cavity. The case was also interesting as showing the kind of cavities in which aneurisms are usually found. It is mostly in connection with bronchiectatic cavities that aneurisms form. The explanation of this is to be found when we consider how the two kinds of pulmonary cavities form. In ordinary caseating phthisis, cavities are formed by the softening of caseous material which is dead matter ready to be removed. The caseous or necrotic process includes lung tissue and blood-vessels, which are removed to whatever extent they have undergone the process. But outside the caseous area there are inflammatory and tubercular lesions, and the blood-vessels have their walls thickened and their calibres diminished before the caseous process overtakes them. It will hence be uncommon to find a pervious dilatable vessel in the wall of such a cavity. In the case of cavities formed by dilatation of bronchial tubes, however, it is very different. The wall of the dilated bronchus is not necessarily thickened and may be thinned. It is to be remembered also that the branches of the pulmonary artery run alongside the bronchi, and a dilatation of a bronchus would probably cause the artery to project in its wall, and would diminish its support. It is quite clear in this case that the bronchial wall is thinned and that the

wall of the vessel, even beyond the aneurism, is very near the internal surface of the bronchus.

As bronchiectasis occurs chiefly in fibroid phthisis, and as this form is pre-eminently chronic, we may expect aneurisms and severe hæmorrhage to occur most frequently in the more chronic cases.

Another point of interest in the case is the fact that the hæmorrhage was arrested and recurred several times. This was explained by the fact that the cavity was small and got readily packed with coagulum so as to still the hæmorrhage. At the *post-mortem* a kind of cap of condensed clot was found covering the aneurism. Otherwise it would be difficult to explain how hæmorrhage from an aneurism with an open mouth like this could cease before the patient bled to death.

## II.—NOTES OF CASE OF INTESTINAL OBSTRUCTION.

By DR. D. N. KNOX.

The patient was a female, aged 49, who had been admitted to the Royal Infirmary on 1st November, 1886, with symptoms of internal strangulation.

These symptoms had first come on a fortnight before admission, but had passed away after lasting several days, except the pain radiating from the umbilicus, which had continued from the first.

On admission patient was suffering from vomiting, with pain in the right iliac fossa and round the umbilicus, and occasional paroxysms of hiccough. The abdomen was much distended, and was tympanitic all over, except in the course of the descending colon, where the note was distinctly impaired; patient also complained of pain on pressure over umbilicus and right iliac region. On manipulation of bowel, movements were easily felt, and were generally succeeded by somewhat painful eructations.

Patient looked fairly well. The expression of her face was not anxious; her pulse was 96 per minute, somewhat feeble, and her temperature normal.

From the time of her admission she began to improve. The vomiting ceased, the abdomen became quite soft and compressible, and appetite appeared to be returning. On the day after admission a large nodular mass was felt at upper part of rectum, and with some difficulty was removed by the finger. It was irregularly oval in shape, about  $2\frac{1}{2}$  inches long by 2 inches in diameter. It was smooth and glistening on its outer surface, and of a dirty yellowish colour; it was easily

broken across, and the fracture was of a dark olive-green colour, studded with shining crystals of triple phosphates. Under the microscope this mass was found to consist of the ordinary constituents of fæces. For several weeks after admission patient continued fairly comfortable; she felt hungry, but generally vomited whatever fluid food she had taken. This vomit was simply tinged with bile, but was not stercoraceous. The constipation continued complete, although everything was done to relieve it. When the abdomen was manipulated with the hand, movements of air were felt, and these were generally followed by foetid eructations. As frequent enemata had been given with the long tube, it is of interest to note that the tube was regularly passed up the descending colon for a distance of 24 to 27 inches.

All the symptoms seemed to point to an obstruction in the course of the ascending or transverse colon, but no tumour could be felt by the most careful manipulation. The advisability of colotomy was next considered; but after consultation it was thought right to continue the attempts, by means of enemata of oil, &c., to relieve what was thought to be either a stricture or an intussusception.

About the beginning of December, after patient had been four weeks in hospital, the vomiting became more frequent and patient became weaker. On account of the vomiting patient took very little nourishment, and began to complain of a severe griping pain in the left side. On 9th December it is noted in the journal that the abdomen appears irregular. There is an arch just above umbilicus, with convexity downwards and considerable bulging in right iliac and lumbar regions, due probably to distention of colon. Severe pain was felt over the whole of abdomen; the tongue was brown in centre, but moist; the urine very dark, but without albumen. Next day the patient was obviously sinking, suffering from severe pain in the abdomen with hiccough, and could not bear to be touched. The bowels now began to move. Fæces at first were solid and clayey, then they became liquid, and for the last few hours of life were passed almost continuously and involuntarily. Patient died on the 11th December.

The sudden exacerbation and intensely painful character of her sufferings immediately before death left no doubt but that perforation and peritonitis had taken place.

*Dr. Newman* said that at last meeting of the Society he had shown the specimens from the case just referred to by *Mr. Knox*, while they were in the fresh state, so that the members might have an opportunity of judging of them for them-

selves. The principal facts of the case, as revealed by the *post-mortem* examination, were these:—Old stricture at the splenic flexure of the colon; great distention of the large intestine above the point of constriction; acute inflammation of the colon; sub-serous hæmorrhages in its wall, and several gangrenous patches with perforations; acute peritonitis; interstitial nephritis of both kidneys, with cysts in the left kidney.

The thoracic organs were practically normal, with the exception of slight calcification of the aorta and of its valves. The peritoneum was in a state of acute inflammation, and the loops of bowel were covered and united together by recently formed lymph; most abundant in the region of the large bowel. Beyond the inflammation of its serous surface the small intestine appeared to be normal, but the colon, from the caput cæcum to the splenic flexure, was greatly distended by fluid fæces containing a large quantity of gas intimately mixed with it. Above the stricture the colon measured about six inches in diameter, while below that point it was about the size of normal small intestine. At a point corresponding to the middle of the transverse colon, and on its anterior aspect, there was a large irregularly shaped slough, including about three-quarters of the circumference of the gut. This gangrenous area measured about  $2\frac{1}{2}$  square inches, and at one point within it the bowel had become perforated, and a quantity of fluid fæcal matter had escaped into the peritoneal cavity. Besides this large slough there were a number of smaller ones, the most important being one situated immediately above the stricture, so that the distended bowel had become almost completely separated from the part below the stricture. The mucous membrane was slightly invaginated at the point of constriction, but there is no evidence of any fibrous band, new formation, or cicatrix, to account for the sudden diminution in the calibre of the bowel, and the obstruction to the passage of fæcal matter.

The pathology of this case is obscure, but two theories might be offered in explanation. The more common causes of obstruction to the calibre of the intestine, such as growths, the contraction of cicatrices, impaction of masses of fæces, incarceration, or the formation of bands, must be set aside as out of the question. The only two explanations are, first, the supposition that there was an intussusception which relieved itself spontaneously, but not completely; or, on the other hand, permanent adhesion may have occurred in an

intussusception followed by union between the outer tube and the inner, while the middle or invaginated portion, becoming gangrenous, separated, and the continuity of the colon was restored with the loss of the invaginated part. Against this latter hypothesis, however, we have the fact that the intestine at the point of and below the constriction showed no evidence of old inflammatory changes. It was not even bound down to the abdominal wall more firmly than normal. The second explanation which may be offered is that there was a congenital malformation at the union of the transverse and descending colon, and that while the lower part of the tract was abnormally narrow, still it was sufficiently large to allow the contents of the bowel to escape. This idea is to a certain extent supported by a fact which has not been referred to—namely, that in the large intestine a number of large plum stones were found, and it is quite possible that although none of these were actually found impacted in the constricted part, one of them might have at some time become arrested in its passage downwards and acted as the exciting cause of the disease from which the patient died. Dr. Newman said that he simply advanced these theories as the most probable explanation of this very obscure case. If the malformation was congenital, certainly it was a very unusual one.

*Dr. G. Buchanan* said that he would not venture to give any opinion as to the pathology of the case, but taking the fact that the patient had suffered from obstinate obstruction of the bowel for 50 days without any evidence of any malignant or inflammatory change, the presumption was that the obstruction was mechanical; and taking the fact that the site of the obstruction was presumably, as it turned out to be, located somewhere in the lower half of the colon, this was just a case in which right lumbar colotomy might have been performed with the result of prolonging the patient's life. He stated that for himself he had grave doubts as to the propriety of performing colotomy in any case of malignant tumour of any part of the colon, or even of enterotomy in cases unavailable for colotomy. The subsequent state of the patient during the remainder of life was not to be desired. But in any case in which the probability was of mechanical obstruction, operation was justifiable and even to be recommended. And this was just one in which he would have been inclined to perform lumbar colotomy on the right side.

*Dr. Joseph Coats* said that there was undoubtedly in this case a localised lesion. The region of the stricture could be

felt to be denser and thicker than the parts above and below, this dense part being about an inch in length. On cutting into this part from without there was a dense fibrous thickening discovered, apparently of the middle coat, and not extending to the mucous membrane. The precise nature of the lesion might be doubtful—possibly an old ulcer produced by a foreign body—but the fact of this localised thickening rather disproves the theory of congenital stricture, which on other grounds had been objected to.

*Mr. Maylard* said—I might, Sir, allude to a case which came beneath my own observation, and though differing in many respects from the one described, may nevertheless throw some little light upon it. It was that of a patient admitted into the Western Infirmary for an abscess in the abdominal parietes. She died shortly afterwards from acute peritonitis, and at the *post-mortem* it was found that the abscess communicated with the dilated part of the transverse colon immediately in front of a stricture situated over the splenic flexure. Numerous strictures of different degrees of contraction existed throughout the bowel, and also several circumferential ulcers in various stages of cicatrisation, so that no doubt existed as to the cause of the stricture at the region corresponding almost exactly to that in the case under discussion. Was Dr. Newman certain of the freedom of the contracted area from anything of an inflammatory nature? It was difficult to accept a congenital view of the constriction considered as arising in development. The intestine from the cæcum to the rectum was uniformly developed, and for a stricture to have its origin in development the only likely spot would be at the junction of the omphalo-mesenteric duct with the ileum. The large areas of gangrene seem an interesting feature in the case. In the one observed by myself, the dilated portion of the intestine—that just in front of the stricture—possessed numerous small ulcers, and it was the perforation through one of these that led to the acute peritonitis which carried off the patient.

### III.—CASE OF PYO-PNEUMOTHORAX CAUSED BY RUPTURE OF LUNG FROM STRAIN UPON OLD PLEURITIC ADHESION.

BY DR. ALEX. ROBERTSON.

J. A., age 21, printer, admitted into Dr. Robertson's ward, Royal Infirmary, on 16th November, 1886.

Patient's illness was stated to be of about three weeks' duration before admission. The leading symptoms were cough,



slight dyspnœa, muco-purulent expectoration with night sweats and considerable emaciation. There was dulness on percussion over right apex in front and behind, and also slight dulness at right base posteriorly. Moist râles at right apex. Three days after admission, quite suddenly, great distress of breathing occurred, and examination revealed the usual signs of pneumothorax. The axillary temperature was  $104^{\circ}$ , and the fever heat continued high on successive days. On the 22nd, the indications of fluid in the pleural cavity were clear, and it being considered that the fluid was probably of a purulent nature, after consultation, Mr. Clark performed the operation of paracentesis. A large amount of pus was withdrawn, the pleural cavity was washed out with solution of perchloride of mercury (1:5000), and a drainage tube afterwards introduced; considerable relief was obtained at the time, but the secretion of pus continued in large quantities, notwithstanding the drainage and the injection of the perchloride solution every second or third day. The patient gradually became weaker, and death occurred on 4th January, 1887.

Dr. Newman remarked that this case was of special pathological interest on account of the mode of production of the pyo-pneumothorax on the right side. The condition revealed by the *post-mortem* examination was as follows:—The left lung was firmly adherent throughout and its apex was consolidated and contained numerous caseous masses and small irregular cavities. The right lung was free, except at a point corresponding to the fourth rib, about 4 inches from the costal cartilage; there it was firmly adherent to the chest wall over an area of about a square inch. Immediately below this adhesion there was a small opening in the lung, through which air passed into the right pleura. The lung was collapsed, and the pleural surfaces were greatly thickened, and covered by a purulent membrane. There is no consolidation or other organic disease of the lung. The left pleural cavity contained about 20 ounces of pus. The pneumothorax in this case was doubtless caused by a rupture of the lung resulting from a strain upon the firm but localised pleuritic adhesion.

Dr. G. Buchanan said that Dr. M'Vail's suggestion reminded him of a case he had seen when clerk in the fever wards in the Royal Infirmary in former days. It was a case of acute pleuro-pneumonia, ending in pneumothorax, which terminated fatally. At the *post-mortem* examination a tough band of lymph was found adhering by one extremity to the pleura costalis, the other to the surface of the lung, where at its

attachment it expanded into a little pyramid like a night cap. This had been torn off from the lung, leaving a distinct aperture which communicated with a bronchial tube and explained the pneumothorax.

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## GLASGOW SOUTHERN MEDICAL SOCIETY.

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SESSION 1886-87.

MEETING XI.—24TH MARCH, 1887.

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MR. EDWARD MACMILLAN, *Vice-President, in the Chair.*

### ON THE TREATMENT OF INFLAMMATORY AFFECTIONS OF THE JOINTS.

By DR. D. N. KNOX. (See p. 81.)

*Dr. James Dunlop* concurred with *Dr. Knox's* views and directed special attention to the treatment of hip-joint disease. He grouped cases of this affection into three classes:—1. Those without any marked strumous diathesis. 2. Those occurring in patients usually under twelve years of age in whom both the constitutional diathesis and local symptoms are present. 3. The large number of cases with severe local mischief in addition to the constitutional diathesis, amongst which are included cases of what is usually called "white swelling." He intended to limit his remarks to the first two groups of cases. A simple case of hip-joint disease readily divides itself into three stages:—1. The child simply limps and there is tenderness over the joint. In this stage we can hope to effect a perfect cure by local and constitutional treatment. 2. Apparent lengthening of the limb takes place and suppuration may ensue, but even in this stage a cure may be brought about with a permanent limp. 3. After suppuration, apparent or real shortening occurs, but recovery to a certain extent, is still possible. To obtain a desirable result, *Dr. Dunlop* recommended that the patient should be put to bed with the limb extended, and that leeches and fomentations be applied to the joint. As soon as the acute symptoms had subsided, the limb should be placed in a splint, which would enable the patient to move about without moving the affected joint or placing the slightest weight upon it. The best apparatus for this purpose,

he thought, is Sayre's splint, a modification of Thomas's; and if counter-irritation be required, he considered the actual cautery the most valuable. Specimens of the various splints were shown by Dr. Dunlop, who suggested that the hospital authorities might lend them to poor patients after they returned to their homes, as the treatment required to be carried out for a very long time.

Dr. Park briefly referred to the strumous diathesis, and gave as his opinion that since scrofula is so rarely seen in old people, it is largely due to a prolongation of the foetal conditions of the tissues.

Dr. Pollok held that tuberculosis and struma are identical. He had found great benefit from the use of alcohol in all strumous diseases, and had obtained good results in the treatment of joint affections by means of rest, depletion by leeches, fixture of the joint by plaster of Paris bandages, in addition to the administration of preparations of iron and cod liver oil.

Dr. Dougall considered that struma, like phthisis, is hereditary, and that the two diseases are pathologically identical. He remarked that bacilli had been found in strumous joints, and held that the strumous diathesis may give rise to cancer. Case of struma, he thought, might arise *de novo*, just as a plant withers and dies when removed from pure air.

Mr. Henry E. Clark's hospital experience of joint disease had been sad and unsatisfactory. The climatic and social conditions under which we live, he thought, were the cause of this. He noted that treatment of such affections was very satisfactory in the Children's Hospital in Manchester, which is situated in the country and built upon the pavilion system. Satisfactory results had also been obtained in the Alexandra Hospital for Hip-joint Disease in London, in which patients are allowed to remain for any length of time. Mr. Clark held that, although pathologists hold that struma and tuberculosis are the same, yet that is not the case in the hospital wards. He had seen cases over a long period of time which did not develop phthisis. With regard to the treatment of morbus coxæ, he pointed out that the great difficulty is to obtain fixation of the joint for a long enough time, which should be four to six months. Mr. Clark referred to the very acute and dangerous affection called "epiphysitis," consisting of inflammation of the upper epiphysis of the femur or of the immediate neighbourhood of the epiphysial line. In the treatment of hip-joint disease, he thought Thomas's splint the best appliance, it keeps the limb in extension, and is very

light; besides it is not expensive. Excision in order to be successful must be done early.

*Dr. Glaister* remarked that parents and guardians do not attach sufficient importance to the symptoms of hip-joint disease, and held that proper treatment cannot be carried out in the ordinary homes of the patients, and that a hospital for the disease is desirable. He believed in the identity of struma and tuberculosis.

*Dr. Knox* replied, and explained, that he regarded cases as cured which had been seen early and completely restored as well as those cases which recovered with ankylosis or deformity. He thought it objectionable to use leeches in the treatment of inflamed strumous joints as the vitality is already lowered. By the word "cancer," he understood *Dr. Dougall* as meaning a sarcomatous disease appearing below the joint, as that affection might arise from an injury.

*Dr. Dunlop* also replied.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

### NERVOUS DISEASES.

BY DR. G. S. MIDDLETON.

**Multiple Neuritis and its Relation to Certain Peripheral Neuroses.**—The following is an abstract of the Middleton Goldsmith Lectures, delivered under the direction of the New York Pathological Society, by *Dr. Allen Starr*, Professor of Nervous Disease (see *Boston Med. and Surg. Journal*, 3rd, 17th, and 24th February, 1887):—

Probably the first description of symptoms given was by *Dr. James Jackson*, of Boston, U.S.A. (*New England Journ. of Med. and Surgery*, vol. xi, p. 351). In a paper "On a Peculiar Disease Resulting from the Use of Ardent Spirits," which he termed arthrodynia, he gives a description, which even now can hardly be improved upon, of what we now know to be one form of multiple neuritis.

The next observations of importance were made by *Magnus Huss*, who, in his work upon *Chronic Alcoholism* (1852) gave a very complete description of alcoholic nervous symptoms, dividing the case into paralytic, anæsthetic, convulsive, epileptic, and hyperæsthetic forms.

In *Duchenne's Electrization Localisée* (1855) a number of cases are grouped together under the title "Paralysie Générale Spinale Subaiguë Ascendante." In the only *post-mortem* on such a case that he had the opportunity of making, he found no spinal lesion, but he judged that the symptoms indicated an ascending degenerative process there. His hypothesis appeared to be verified by succeeding observers, mainly of the French school, until various observers (*Dumenil, Gaz. Heb.*, 1864, p. 203, and 1866, No. 4; *Eichhorst, Virchow's Arch.*, Bd. 69, p. 265, 1876; *Joffroy, Arch. de Phys.*, 1879, pp. 172-193: and

Leyden, in 1880) published cases in which careful *post-mortem* examinations proved that in some at least of the cases the disease affected the peripheral nerves, and not the cord, though in the later stages of a prolonged case spinal lesions might arise. Leyden's cases raised a very general discussion, but it is now not questioned that the later opinion is correct.

[The lecturer gave a minute account of the pathological changes found in the nerves, with full discussion of the various theories advanced to explain them. He concluded by stating that there were evidently two main forms of neuritis, viz., parenchymatous and interstitial, with a third form, described by Gombault (*Arch. de Phys.*, 1873, p. 592, and *Arch. de Neurol.*, i, 1, as occurring in cases of lead poisoning, and termed by him Segmental Peri-axillary Neuritis.]

By the end of 1883 the symptoms of the disease had been recognised as constituting a distinct clinical picture; hypothetical lesions in the spinal cord had been abandoned. Since then, about 100 cases have been recorded, besides numerous cases of anomalous affections, such as "numb fingers," intermittent paralysis, and other affections of sensibility, which may prove to be slighter forms of peripheral neuritis.

Among the cases of multiple neuritis it is possible to distinguish—

I. Toxic cases, due to poisoning by alcohol, arsenic, lead, and bisulphide of carbon.

II. Infectious cases, due to the direct action upon the nervous system of the infectious agents producing diphtheria, variola, typhoid and typhus fevers, severe malarial fevers, and tuberculosis, to which must be added the agent causing the epidemic form of neuritis known as Kakké or Beriberi.

III. Spontaneous cases, due to uncertain causes, among which cold and exposure to damp and wet and over-exertion may find a place.

*Toxic Cases.*—Dr. Starr gives a full description of the clinical features of these cases, but does not profess to add any new details. He draws attention to the fact that many of the cases of alcoholic and arsenical neuritis bear a very close resemblance to cases of locomotor ataxy, but believes that careful investigation of the symptoms, and especially of the history, will make the true nature of the case quite evident. He states that the "Argyll-Robertson" pupil is not a symptom of *alcoholic* neuritis, and that the mental condition in this disease is often very characteristic, "and hardly ever wanting. There is at first excitement rising to the degree of active delirium, with illusions and hallucinations of the various senses; there is insomnia, loss of memory, and lack of power of attention or concentration. The indifference to bodily wants may be so great as to lead to uncleanness. It is useless to get any reliable history of their illness from these patients, as their statements are unintelligible or unreliable." Notably "they will relate occurrences as having happened recently, with much elaboration of detail, when, as a fact, the story is entirely a product of their own imagination." "The course of alcoholic neuritis is quite uniform. After a sudden onset the symptoms rapidly advance to a high degree, which is reached in a week or two from the beginning of the paralysis or ataxia. Then they may increase further, and cause death by respiratory paralysis. Usually they remain stationary for a time, and then gradually subside, the entire duration being from two months to a year. Individual muscles regain their power, tone, and firmness and electrical reaction slowly, and during recovery the tingling and numbness in hands and feet may be severe. In a few cases the muscles become contracted, and permanent deformities, only to be overcome by long-continued massage or by operative measures, develop." Recovery, especially if rapid, is usually followed by early resumption of drinking habits, which often renew the disease.

In the case of *lead* the paralysis is often more local. On the other hand, while it has been clearly shown that lead may cause peripheral neuritis without affecting the spinal cord, it has also been shown by numerous autopsies that in many cases the spinal cord is implicated in the morbid processes.

*Infectious Cases.*—Of these the most common are cases of post-diphtheritic paralysis. Paralysis after variola is very rare, but Dr. Starr records in these

lectures one case in which *post-mortem* examination (the patient died of inter-current pneumonia) showed marked degenerative changes in the peripheral nerves and the corresponding muscles. With regard to the other diseases included under this heading the case is not so clearly made out; in syphilis it is as yet even more doubtful. The pathology of Beriberi seems pretty well established by recent researches.

**SYMPTOMS** are numerous and varied.

*Sensory Symptoms* are the earliest to appear and the last to disappear. All kinds of paræsthesiæ may be found beginning in the extremities, often so severe as to be very distressing. They extend more or less towards the body as the disease advances, but at its acme usually disappear. Their recurrence is usually a prelude to recovery. There is usually some pain, resembling, but seldom so severe as that of locomotor ataxy. There is marked tenderness of nerves and muscles, and occasionally some swelling and œdema. Hyperæsthesia is often an early symptom, usually followed by some anæsthesia, both often in limited areas. Transmission of sensation of pain and temperature is always delayed, though they are clearly felt. Sense of pressure is impaired. The state of the muscular sense varies greatly, but it may be as much impaired as in advanced locomotor ataxy. The special senses are usually normal.

*Motor Symptoms* are as marked and important as the sensory. Fatigue on exertion, and a feeling of weakness increase till complete paralysis is reached, often in two weeks, usually within two or three months. The distribution of the paralysis is not uniform at first, but later, all the muscles moving the wrists, hands, ankles, and feet are involved. In a few cases the muscles of the thighs and upper arm, and even those of the trunk become involved, and then the disease resembles Landry's paralysis. But in the latter disease the paralysis invades the legs, thighs, trunk, and arms in the order given, while in multiple neuritis the trunk is invaded last. "The paralysed muscles are relaxed, flabby, and atrophied; they may or may not lose their mechanical irritability, but their normal tone is always lost, and hence the so-called tendon reflexes are abolished. To the electric current their excitability is very rapidly and markedly changed; but the conditions which have been observed are quite various. Sometimes there is a simple diminution of excitability, and then a very strong Faradic or galvanic current is needed to produce contractions. Frequently all Faradic excitability is lost, and then the muscles re-act to a galvanic current only. In this condition it may require a very strong galvanic current to produce contraction, and this fact is quite pathognomonic of neuritis. For in anterior polio-myelitis, when the muscles respond to galvanism only, it does not require a strong current to cause a movement until some months after the invasion. The action of the different poles is not uniform. In many cases the contraction of the muscles, when stimulated with the positive pole, is greater than when stimulated with the negative pole, and the contractions may be sluggish. Then the reaction of degeneration is present. But in some cases the normal condition is found, and the negative pole produces stronger contractions than the positive pole. If the muscles that are not paralysed be tested, the same electrical changes may often be discovered in them. A loss of Faradic irritability and a marked decrease in the galvanic irritability of the muscle and nerve are, therefore, important symptoms of multiple neuritis. And as the disease goes on to recovery, a gradual increase in the galvanic irritability occurs, a fact which is often of much aid in prognosis if careful measurements of the strength of the current used be made."

*Abnormal Position*, e.g., "dropped wrist" may be produced by the paralysis.

*Vaso-Motor and Atrophic Symptoms* are less constant. Except œdema, profuse perspiration limited to affected part, and glossy skin, few have been noted.

*Condition of the Sphincters*, normal—often an important symptom diagnostically.

*Temperature*.—Sometimes there is sudden onset, with rigor, and temperature of 103° to 104·5° F., which usually subsides after some days and does not

recur. Sometimes there is persistent slight elevation ( $1^{\circ}$  to  $1.5^{\circ}$  F.) of temperature.

*Pulse.*—Often rapid. "A rate of 90 need give no alarm, but if it runs up to 140, and becomes irregular, there is reason to believe the disease has attacked the vagus nerve, and then the prognosis becomes serious, though not by any means hopeless."

*Duration.*—In 25 cases this varied from 2 months to 16; average, 7 months.

*Sex.*—Most common in males, except in the alcoholic form.

*Age.*—Recorded cases, except the diphtheritic ones, do not include children. Dr. H. Chapin (New York) has, however, recorded 4 cases which were such as is usually called infantile paralysis, but in which the presence of sensory symptoms and steady progress to recovery points to the existence of multiple neuritis.

*DIAGNOSIS.*—The distinctions between neuritis and anterior polio-myelitis, locomotor ataxia and diffuse myelitis, and the recognition of the complication of neuritis with myelitis, and of locomotor ataxy with neuritis, are important.

*Neuritis v. Anterior Polio-Myelitis.*—"Atrophic paralysis, with R. D. and loss of reflex, is common to ant. polio-myelitis and some cases of neuritis. In the latter, a more gradual onset, preceded and attended by numbness and pain, tenderness of nerves and muscles, and the persistence of sensory symptoms will remove all doubt. When these are not clearly marked, the distribution of the paralysis is symmetrically situated in the muscles, especially if these are supplied by single nerves, and the extension to muscles in other nerve domains, rather than the simultaneous affection of physiological groups of muscles, will point to neuritis." The progress towards recovery finally settles the diagnosis.

*Neuritis v. Locomotor Ataxy.*—"Ataxia, loss of knee-jerk, sensory disturbances, including loss of muscular sense, Romberg's symptom, and optic neuritis, are common to both. In the latter, the relatively rapid onset of the ataxia, closely following upon the sensory symptoms; the prominence of numbness and anaesthesia, rather than of lightning pains; the extreme anaesthesia and analgesia; tenderness of muscles and nerves; usually actual paresis, with atrophy and R. D.; and the absence of bladder and sexual symptoms, will point inevitably to the diagnosis." Furthermore, the ataxic form of neuritis only occurs after diphtheria or alcoholic or arsenical poisoning. Progress toward cure completes the diagnosis.

*Neuritis v. Myelitis.*—"These have many symptoms in common. Duchenne's "paral. générale spinale subaigue ascendante" (*v. supra*) is very rare, and some have asserted that the cases are really instances of neuritis. Points of difference, however, are "in neuritis, affections of micturition and defecation do not occur; girdle sensation is rarely mentioned; bed-sores and cystitis are absent; paralysis attacks the muscles in a different order (*v. supra*), and as a rule, only distal parts are affected, except in rapidly fatal cases; there is usually some ataxia, and loss of muscular sense is marked; there is tenderness of nerves and muscles, but no tenderness to pressure or heat over the spine."

Other nervous diseases are easily distinguished.

*Neuritis Complicated with Myelitis.*—Recognition important as bearing on prognosis, and because the combination has been found *post-mortem*.

(1.) While the case is advancing such recognition is impossible; a later stationary period need cause no alarm; but if after four months there is no improvement, the probability is either that the cause has not been removed or that incurable myelitis has supervened. (2.) Positive symptoms of this are increasing weakness, more rapid atrophy and increased loss of galvanic irritability in muscles, loss of sensation of pain and temperature, decreased paræsthesia, but increased anaesthesia, paralysis of sphincters, the formation of bed-sores, bullæ, or furuncles, and cystitis. (3.) If power gradually returns; if anaesthesia decreases and is succeeded by paræsthesia, however unpleasant; if galvanic and Faradic excitability increase; if tenderness of muscles and nerves decreases; and if glossy skin gradually becomes normal, it may be stated that no complication of myelitis has occurred, and that recovery, however tardy, will ensue.

*Locomotor Ataxy Complicated with Neuritis.*—"There are certain symptoms which develop in some cases of locomotor ataxy, but not constantly, which may be looked upon as accidental, and thus traced to neuritis. Such are the appearance of plaques of anæsthesia and analgesia of limited area, local muscular weakness or paralysis, trophic changes in skin, nails, joints, bones, and teeth, and possibly visceral and laryngeal crises. Peripheral neuritis has been found *post-mortem* in nerves supplying parts thus implicated. It is, therefore, reasonable to conclude that in any case of locomotor ataxy in which these symptoms develop, we have to deal with a posterior sclerosis which is complicated by a peripheral neuritis. And here again the distinction has a bearing upon prognosis, for the symptoms of the accidental kind may pass off, while those due to the central lesion will remain. While these conclusions of Pitres and Vaillard are of importance, it must be noticed that their claim that trophic disturbances are due to a complicating neuritis is by no means substantiated by the history of cases of multiple neuritis, in which, as we have seen, trophic disturbances of the varieties mentioned do not occur.

**PROGNOSIS.**—In uncomplicated cases, especially when the cause can be controlled, it is good, but the general state of the patient must be taken into account.

**TREATMENT.**—In most cases, the cause being removed, expectancy would succeed eventually, but progress can always be aided by suitable treatment. Early, in acute cases full doses of salicylates (salicin, acid, or soda salt) with bromides or even morphine, as indicated, do much good, and protection of painful parts with either hot or cold applications (whichever are found to succeed) give great relief. Syphilitic cases should be treated by simultaneous administration of iodide of potass and mercurial inunction, and malarial cases with quinine or Warburg's tincture; arsenical or lead poisoning on general principles, including, of course, the iodide. Alcoholic cases are very difficult to manage, and must be treated very much in the same way, with regard to stimulants, as cases of delirium tremens.

In the chronic stage, arsenic and strychnia should be used alternately, each two weeks at a time, with massage, baths, douches, and above all, electricity. Mild galvanism should be used daily for about ten minutes to each limb. When recovery has advanced sufficiently for it to take effect, Faradisation should also be employed. Contractures, if present, may be removed by massage, or if that fails, by surgical means.—D. M'P.

## SURGERY.

By MR. A. E. MAYLARD.

**Turpentine Injections in Fistulous Sores.**—Cecchini gives details of numerous cases of atonic fistulous sores rapidly cured by injection with oil of turpentine (*Annali. univ. di med. e chir.*, August, 1885). The drug has a powerful stimulant effect upon granulations, is strongly "antimycotic," and has no bad effects. In some cases where it caused more pain than usual, it was diluted with almond, or olive oil. The injections were made with a short pointed syringe so as to fill the cavity of the sore thoroughly each time, and were usually repeated at intervals of three days. The details are given of cases of fistula in ano, caries of the petrous bone (in children), dental fistulæ, fistula of Steno's duct, and other sinuses. Anatomical tubercles upon his own hand, which iodoform failed to heal, healed rapidly under a dressing of lint wrung out of turpentine.—(*Centralbl. f. Chir.*, 2nd January, 1886.)—D. M'P.

**Subluxation of the Head of the Radius in Children.**—This injury is probably a very common one, overlooked in so-called "sprain of the elbow." It is not surprising that it is often overlooked, when we consider how



easily it is produced, how little deformity or limitation of movement it causes, how very likely the manipulations of examination by a surgeon are to reduce it, and how immediate the recovery of full use of the limb is. It has been very well described, with cases and dissections, by J. Hutchinson, Jun., F.R.C.S. in *Annals of Surgery* for August, 1885, and by him and S. H. Lindeman, M.R.C.S., in *Brit. Med. Journal* for 5th December, 1885, and 2nd January, 1886. It is claimed, however, that it was previously described by Hodges (*v. Boston Med. and Surg. Journal*, 18th September, 1862, and *Ranking's Abstracts*, 1863, vol. 1, p. 201), by Goyraud of Aix (*L' Union Medicale*, 23rd November, 1861), and by Streubel, in 1850, (*vide Richter in Centralbl. f. Chir.*, 6th March, 1886.) Goyraud, however, while giving an accurate clinical account of the symptoms, believed them to be "due to a displacement of the interarticular fibro-cartilage of the wrist in front of the carpal extremity of the ulna" (*loc. cit.*) The following description of the injury, abstracted from a paper by Dr. H. W. Cushing, Boston, U.S.A., in the *Boston Med. and Surg. Journal*, 23th January, 1886, may be compared with those of Messrs. Hutchinson and Lindeman.

"This injury is most commonly met with in children under five years of age. In most cases it is the result of sudden traction on the extended upper extremity, such as a child receives who, led by the hand, stumbles, and is prevented from falling by the jerk with which the nurse restores it to an upright position, or when it is suddenly lifted up from the ground by one hand, or even when sharply drawn forward by one hand when walking. It may also be, but more rarely is, caused by a blow or fall upon the back of the semi-flexed elbow, forcing the head of the radius directly forward.

"The anatomical arrangement of the articulation seems to indicate that the lesion is produced as follows:—The force applied at the wrist by traction on the hand affects the radius mainly, since that bone occupies a greater portion of this joint, and is transmitted through it to the elbow, where the radial head is held in position by a group of ligaments, of which the anterior and external lateral act as guys to the orbicular, which in turn holds the radius in its normal position. The orbicular is itself attached to the radial neck, and this is the weakest point in the structure. In the position of the joint in which the accident occurs, the posterior fibres of the external lateral ligament are relaxed, and the tense anterior ligament, with possibly the anterior fibres of the external lateral, is the one left to support the orbicular against the traction from below, which it is strong enough to do. The force is thus directed to the insertion of the orbicular about the radial neck, at the interior portion of its circumference; this yields, and a subluxation is the result. The direction forward which the radial head almost invariably takes, is probably due to the yielding of the ligament at this point, and possibly also to the traction of the bicipital tendon.

"The clinical history is that the patient is injured in a majority of cases, as above described; often a click or thud is noticed, or something is felt to give way, by the person holding the child's hand. The patient cries with pain, and the arm drops motionless to the side, with the elbow semi-flexed, and the hand partly pronated. Attempts to use the latter cause marked pain. The attendant is frequently puzzled to determine whether the wrist, elbow, or shoulder is the seat of trouble, the latter being often designated as the location of the lesion. Surface markings are usually absent. No deformity exists, except when the subluxation is marked, in which case, the projection of the radial head causes a slight prominence below the external condyle on the anterior surface of the elbow. Tenderness on the head of the radius is the most constant symptom. By careful manipulation, the projecting edge of the radial head can be felt, but it is easily overlooked, and the joint appears normal, as far as its anatomical details are concerned. In some cases flexion and extension are painful, but usually supination only causes discomfort. In most cases supination alone is limited, apparently by a mechanical obstacle. If the subluxation is reduced, either spontaneously or unconsciously by the examiner, during passive motion or other manipulation, all symptoms

suddenly vanish, and nothing remains except slight tenderness over the radial head, which soon disappears. If it is not reduced the arm drops back into its former position, a symptom considered by Goyraud as pathognomonic.

"Reduction is usually accomplished without difficulty by first semi-flexing the elbow, in order to relax the bicipital tendon, and approximating as closely as possible the displaced ligament and the edge of the radial head. Then with the forearm grasped by the left hand, supinate steadily with the right, while the left thumb over the radial head pushes in a backward direction. With a slight click or thud, the obstacle to supination suddenly yields, the head is felt to slip back into its place, and the movements of the joint become normal. Hutchinson claims that flexion with *pronation* is an easier and surer way to reduce the displacement; for the part of the radial head opposed to the curve of the orbicular ligament during pronation, is its thin rounded edge, over which the ligament easily slips; while full supination, by presenting a deep edge with a rectangular contour, renders reduction more difficult."

[Undoubtedly either supination or pronation, with flexion and direct pressure upon the displaced bone, reduces the displacement easily, but pronation appears the preferable method, not only because of the anatomical fact noted above, but also because it causes the child less pain. Supination is usually the only painful movement, and surely should be avoided when pronation succeeds as well, or better.—D. M'P.]

Unrecognised and unreduced subluxation of this form probably usually gives perfectly useful joints. But there may remain permanent slight deformity or enlargement of the elbow, and in weakly children, or where there has been much bruising at the time of the accident, inflammatory changes may supervene, which may end in serious changes in the joint. Instances of this are given by Hutchinson, Lindeman, and Cushing.—D. M'P.

**Treatment of Contraction of the Palmar Fascia (Dupuytén's).** By Theodor Kocher, Bern. (*Centralbl. für Chirurgie*, 2nd July, 1887).—After treating of the etiology and histology of the affection, Professor Kocher passes on to discuss the various methods in practice adopted to relieve the contracted condition, and concludes by advising excision of the fibrous constricting tissue. This should be done through a single longitudinal incision, and in old cases where the cutis has become so intimately adherent to the underlying fibrous bands, a piece of the former should be excised. To attempt to excise under these conditions only the contracting tissues would leave the overlying skin too thin to live, and thus the wound would heal by granulation, rather than by primary union, which is essential. Gersuny has also practised this operation with very good results.

**Nephro-lithotomy.** By Thomas Jones, M.B., B.S., F.R.C.S. (*Medical Chronicle*, June, 1887).—This case is quoted to show the uncertainty derived from exploring the kidney for stone with a needle, as compared with the safe and sure mode of diagnosis by means of renal incision and digital exploration. J. W., aged 17, was admitted into the Manchester Royal Infirmary, on 28th March, 1887. In June, 1886, he noticed for the first time during micturition that his urine was of a "deep cloudy red colour." The hæmaturia existed for several days, after which he passed perfectly clear urine for a week or so. He continued, however, to pass blood in his urine at short intervals up to the date of his admission. The quantity of blood present did not appear to be influenced by previous exertion. Before July, 1886, he had had no abdominal pain, with the exception of some colicky pains in the epigastrium, to which he had always been subject; from this time forward he suffered from more or less constant pain in the left lumbar region. The pain was not greater when standing, walking, or running, than when lying down. At times, usually between 4 and 8 A.M., it came upon him in sharp paroxysms, and then, as he described it, "fairly doubled him up." Patient experienced

no pain in micturition, but lately required to pass his water more frequently than normally, having at times to get up at night for this purpose. At the time of admission he was perfectly free from pain, but on pressure over the left lumbar region, an area of great tenderness was discovered, situated  $2\frac{1}{2}$  inches internal to the anterior superior iliac spine, and  $\frac{3}{4}$  inch above it. No tenderness over either kidney in front, but considerable tenderness over the left renal area behind. On bimanual palpation no enlargement of the kidney was detected. The amount of blood present was at first very large, but gradually diminished, so that by a week after admission the urine was perfectly clear, and contained a mere trace of albumen. Nephro-lithotomy was performed on 7th April. The kidney exposed, the forefinger was passed round its vertebral edge to the anterior surface, and the organ grasped between the finger and thumb: nothing, however, could be felt. The kidney was then carefully explored by systematic puncture with a long needle, passed towards the pelvis, but no calculus could be found. An incision sufficiently large to admit the tip of the index finger was then made through the kidney substance into the pelvis by means of a fine bistoury. On introducing the finger, a small stone was discovered, firmly lodged in one of the superior calyces. Small straight lithotomy forceps were introduced, and the stone then removed. The hæmorrhage from the kidney was free, but soon stopped, after the application of pressure with antiseptic sponges. The patient made a good recovery. In his remarks upon the case, Mr. Jones points out the unusually copious hæmaturia which occurred, suggesting rather a growth than a calculus. The kidney structure bled freely when incised, so that the vascularity of the organ accounted for the previous excessive hæmorrhage. The cut surfaces of the kidney adhered very early, and prevented the urine from escaping through the wound.

[Another feature of interest in this case was the inability to detect the stone by the usual exploring method with a needle. The case also adds another to those of Mr. Bennet May, given in the June abstracts, where incision and digital exploration proved both a safe procedure and a certain mode of diagnosis.—A. E. M.]

**Iodol; an Effective Substitute for Iodoform.** By R. Noris Wolfenden, M.D. (*The Practitioner*, vol. xxxviii, No. v.)—For the purposes for which iodoform is used, it appears to have value quite equal to this body, and is infinitely preferable from the fact of its being a pleasant and nearly odourless body, instead of nauseous and foul smelling like iodoform. Iodol is slightly soluble in water in proportion of 1 to 5,000; it is quite soluble in three parts of absolute alcohol; very soluble in ether, forming a brown liquid, and less soluble in chloroform (50 per cent). It is undoubtedly a fact that no constitutional phenomena follow the long continued use of the drug as an external application, and it is thus free from one objection which can be urged against iodoform. Iodol is undoubtedly antiseptic; its antiseptic power being due to the liberation of free iodine. Added to fluids which readily undergo decomposition, iodol keeps them fresh for months. Mazzoni and Rocchi, who were the first to use the drug, remark that in wounds in which necrobiosis is very pronounced, the surface cleans and commences to granulate in a very short time. They also observed quick amendment in atonic ulcers under its influence, and found it serviceable in lupus. It is quickly remedial in syphilitic conditions. According to Schmidt, it speedily deodorises the foul discharges of cancer of the rectum and uterus. Pick has used it extensively in venereal conditions, chronic ulcers, scrofulous abscesses and lupus, with uniform benefit.

**On the Ultimate Results of the Mechanical Treatment of Hip-joint Disease.** By N. M. Shaffer, M.D., New York, and R. W. Lovett, M.D., Boston. (*New York Med. Journ.*, 21st May, 1887.)—The subject of this interesting and instructive paper is founded on the analysis of fifty-one cases of hip-joint disease occurring in the service of the New York Orthopaedic

Dispensary and Hospital. The patients treated were all of the usual dispensary class, and all treated as out-patients, and with such care and watchfulness as to reflect the greatest credit upon the institution and its officers. Not only were they carefully attended to at the dispensary, but in the event of their failing to appear at the usual weekly visit, they were visited and treated in their homes. To this, as much as to anything, must the success of the conservative measures adopted be attributed.

The aims of the treatment were to overcome by mechanical means any acquired deformity that existed before treatment commenced; to protect the diseased joint from traumatism; to permit the patient to have almost unrestricted out-door exercise; and to maintain that position of the limb which would reduce to the minimum deformity, if ankylosis occurred. To effect these results, a long Taylor traction-splint is applied soon after the first examination, and the parents or some friends of the patient are instructed in the use of the apparatus. The patient, unless recumbency was found necessary to overcome a malposition of the limb, or unless the symptoms were so acute as to demand rest (in which case the patient was visited at home by the out-door visiting surgeon), is allowed almost unlimited exercise in the open air.

Of 51 patients discharged as cured over four years ago, 4 had died, 6 have had relapse, and 41 have apparently been cured of disease.

## INSANITY.

By DR. R. S. STEWART.

**Alcoholism Mistaken for General Paralysis.** By M. Garnier. (*Annales Med.-Psychol.*, March, 1886).—This is an example of the difficulty that sometimes arises in the diagnosis of general paralysis from alcoholism. A carpenter, aged 35, manifested symptoms of general paralysis—embarrassment of speech, tremor of lips and hands, rigidity of the limbs, pupillary inequality, impairment of memory, and grandiose ideas. These, on admission, were of four months' duration, and were attributed to alcoholic excesses and domestic troubles. In two months he improved somewhat, and was discharged on trial. Two years afterwards he was admitted in a state of chronic mania, with incoherence and ideas of persecution, but with less pronounced hesitation of speech and tremor of hands. At the end of fifteen years he was still alive, his mental condition at that time being one of *simple*, as contradistinguished from *paralytic*, dementia.

**Expectation as the Method of Treatment of Delirium Tremens.** By Jules Christian. (*Annales Med. Psychol.*, March, 1886).—During 16 years the writer has had 56 cases under his care, and of these 44 recovered and 12 died. This is apparently a large percentage of deaths, but of these 7 died within 24 hours after admission, and, in the remaining 5, death ensued from complications quite foreign to the disorder (suppurative peritonitis, pneumonia, tubercular enteritis, encephalitis, and acute meningitis). The essential indication to treatment he holds to be, not the induction of artificial sleep by the use of hypnotics, such as chloral or opium, but rather the elimination of the alcohol with which the system is impregnated, and to which the three main symptoms—the insomnia, excitement, and hallucinations—are to be attributed; and the elimination he aims at by the use of prolonged tepid baths and copious draughts of simple fluids.

**A case of General Paralysis with Ataxy of the Left Upper Extremity and Complete Hemiplegia.** By P. Rey. (*Annales Med.-Psychol.*, May, 1886).—A man, aged 38, addicted to alcoholic excesses, began in 1880 to complain of headaches and giddiness, then he became irritable, violent, and indolent, and his memory became weakened. Some months later

he had a fit with loss of consciousness, followed by embarrassment of speech, motor weakness, and troubles of visions, and a year afterwards delusions of grandeur developed. The peculiar features of the case were, a very marked inco-ordination of the left arm, with complete abolition of sensibility and permanent contraction of the middle and ring fingers, and the occurrence later on of convulsive seizures predominating on the left side, and succeeded by left hemiplegia. At the autopsy the alterations observed were opalescence and thickening of the meninges; adhesions of the pia to the underlying cortex, very profound and extensive on the right side, but superficial and disseminated on the left; and a difference in weight between the two hemispheres of 90 grains, the right weighing 355, the left 445 grams.

**An Entire Family Attacked Simultaneously with Demonomania.** By Lapointe. (*Annal. Med. Psych.*, November, 1886. P. 350.)—This family consisted of the father (a tenant farmer), the mother, three sons, and two daughters. With the exception of one son, who was a soldier, all of them were engaged at the work of the farm, and were naturally simple, credulous, and of feeble intelligence. In 1882 they became possessed of the same deluded ideas; they believed themselves poisoned by sorcerers and that the devil was in their clothes; they had hallucinations of sight (they saw the devil incessantly, and sombre-coloured animals), of smell (sulphurous and mephitic fumes), and hearing (imaginary murmurings and door-knockings). Their mental derangement necessitated confinement in the asylum, where they remained for a period of a little under a month. They continued after their discharge in a satisfactory way till 1884, when first the mother became unsettled, accusing imaginary enemies of giving them all over to sorcerers and the devil, then the other members shortly afterwards became affected and all six were again admitted to the asylum, suffering from a form of mental disorder almost identically similar to that which existed on their first admission. After a residence of 5 months they were sufficiently recovered to return to their home.

**Echolalia in the Course of a Mental Affection.** By H. Mabile. (*Annal. Med.-Psych.*, November, 1886).—The summary in this case is, hystero-mania with periods of excitement, followed by periods of depression; delusions of poisoning and of grandeur; sudden changes of personality; muscular jerking; heredity; echolalia. This term echolalia (ἠχῶ, echo; λαλία, form of speech) is the name applied to the symptom which is characterised by the immediate repetition of the words and sentences spoken by another. The peculiarity of this particular case was that the symptom was manifested only in relation to one single individual, the patient's mother, whose words she instantly repeated with the most exact intonation of voice and imitation of gesture.

## DISEASES OF THE EAR.

By DR. WALKER DOWNIE.

**What is Meniere's Disease?**—In the last number of the *Polyclinic* to hand Dr. Burnett, Professor of Otology at the Philadelphia Polyclinic, has an interesting paper with above title.

Ménière, in his description of the aural disease which bears his name, regarded the phenomena of aural vertigo as apoplecticiform, the lesion occurring in the semicircular canals. His description is so comprehensive that according to investigation of subsequent observers it cannot be applied solely to the train of symptoms following a lesion of the semi-circular canals; for lesions in other parts of the ear are competent to evoke the same phenomena.

Dr. Burnett says, "It is highly probable that in every case of ear vertigo the semi-circular canals are irritated and cause the vertigo, but the primary

lesion need not be there. At most they are, in many cases, but the seat of a reflected irritation, originating it may be from undue pressure on the drum head, on the ossicles and mediately, through the labyrinth fluid, upon the ampullæ of the semi-circular canals. But Ménière claimed that in every case of aural vertigo the semi-circular canals were primarily and solely the seat of disease, in which he was manifestly wrong."

Delan published a paper nearly 25 years before Ménière's paper in which he maintained that too often affections of the middle ear were mistaken for diseases of the brain; and he held that aural vertigo and its correlated symptoms (now called Ménière's disease) were due to affections of the middle ear. Similar symptoms may result from irritation in the external ear.

Dr. Burnett, after stating that Ménière's disease, as dependent *solely* upon an affection of the semi-circular canals, rarely, if ever, exists, objects to the name as a general term, but with "aural or ear vertigo" as the general designation proposes "to name the three forms under which it appears as 'external ear vertigo,' 'middle ear vertigo,' and 'internal ear vertigo,' according to the localisation of the irritation in either of those portions of the ear," and he would have "Ménière's disease," if it means anything, and if resolved to retain it, to indicate a form of internal ear vertigo. The phenomenon of aural vertigo depends upon the structure of the auditory nerve. Some of its fibres of origin are closely connected with a mass of motor cells in the bulb, which fibres pass into and are continued in the inferior peduncles of the cerebellum.

"The diagnostician should bear in mind that aural vertigo is the general term, Ménière's disease the particular form; that the former is common, and that the latter is rare; that aural vertigo, from irritation in the external and middle ear, is not usually attended with great or incurable deafness, whereas true Ménière's disease is necessarily attended with profound and permanent deafness. Aural vertigo can be excited artificially, as, for example, by injecting cold water into the external auditory canal; Ménière's disease, on the other hand, must necessarily depend upon an organic lesion. All forms of aural vertigo are paroxysmal, excepting those dependent upon the presence of a foreign substance in the external ear pressing upon the membrana tympani, and upon a morbid growth in or upon the auditory nerve. In the latter there is a permanent, though it may be slight, tendency to reel, or to walk in a one-sided manner.

"As the tumour enlarges the vertigo may become intense and distressing, and the equilibrium is so much interfered with as to forbid walking alone. While this may be considered a form of aural vertigo it is not Ménière's disease, because it is not strictly caused by irritation in the internal ear, and the vertigo is not paroxysmal, but constant."—*The Polyclinic*. Philadelphia. April, 1887.

### *Books, Pamphlets, &c., Received.*

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Lehrbuch der Krankheiten des Rückenmarks und Gehirns. Von Dr. Ad. Seeligmüller, Zweite Abtheilung. Braunschweig: Friedrich Wreden. 1887.

The Mineral Waters of Vichy. By Dr. C. E. Cormack. London: J. & A. Churchill. 1887.

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A Treatise on the Theory and Practice of Medicine. By John Syer Bristowe, M.D. Sixth Edition. London: Smith, Elder & Co. 1887.

- A Practical Treatise on the Diseases of the Rectum. By Alfred Cooper, F.R.C.S. London: H. K. Lewis. 1887.
- Guide to the Administration of Anæsthetics. By Henry Davies, M.R.C.S.Eng. London: H. K. Lewis. 1887.
- An Introduction to the Study of Embryology. By Alfred C. Haddon, M. A., M.R.I.A. With numerous Illustrations. London: Charles Griffin & Co. 1887.
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- Notes on Surgery for Nurses. By Joseph Bell, F.R.C.S.Ed. Edinburgh: Oliver & Boyd. 1887.
- Transactions of the New York State Medical Association for 1886. Vol. III. Concord. 1887.
- The Forms of Nasal Obstruction. By Greville Macdonald, M.D. London: Alexander P. Watt. 1887.

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ORIGINAL ARTICLES.

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OBSERVATIONS ON THIRTY CASES OF ABDOMINAL  
SECTION FOR OVARIAN AND OTHER CYSTIC  
TUMOURS.

By H. C. CAMERON, M.D.

*(Read before the Glasgow Medico-Chirurgical Society.)*

THE notes, which I wish to read to the Society to-night, and which I have thought may serve as the basis of interesting discussion, concern thirty cases of abdominal section performed for the removal of cystic tumours.\* Twenty-eight of these were ovariectomies, and occurred in twenty-seven patients, one patient being twice the subject of operation. Two were retro-peritoneal cysts of large size, presenting difficulties of diagnosis, and still greater difficulties in reference to the operations undertaken for their removal. It may be convenient that I should first refer to them.

CASE XXIX.—Mrs. K., somewhat over forty years of age, was operated on at the Training Home for Nurses, in Renfrew Street, on 25th October, 1884. She was sent to me by a medical friend in a neighbouring town on account of a large thin-walled cyst, which distended the abdomen very thoroughly. Fluctuation was everywhere very distinct on the slightest tap being made on it. It seemed more abdominal than pelvic; indeed, the hands could be pushed

\* For full List of Cases, see end of article.



between it and the pelvis at its lower border. It appeared to be quite freely movable below but somewhat fixed above. It bulged a little in the left loin. Coils of intestine, at different parts over the front of the cyst, were always discovered by percussion, but, as they varied in position, were clearly not closely adherent. It seemed probable that the case was not one of ovarian cyst; and while its position and fixation pointed to the possibility of a connection with the left kidney, there were no other circumstances to support this view. It had been growing for several years. I was assisted in this and in the great majority of my other cases by Dr. W. L. Reid, to whom my thanks are due, not merely for efficient assistance at the operation, but also for that, which I felt sometimes to be of even greater value—viz., advice beforehand as to the condition of the pelvic organs and their relations to the tumour. I cannot overrate the value of Dr. Reid's services to me in this matter. Mr. Maylard has also in most of the cases rendered me valuable aid.

On opening the abdomen and removing the omentum and intestines which lay in front of the cyst, it was soon evident that it had no pelvic connection whatever. Universally covering it, but easily separated, there was a thin membranous expansion, which really was a second layer of peritoneum, the cyst lying entirely behind and outside of that membrane. The fluid contained in it was very thin and flowed freely through the cannula. In five or ten minutes it coagulated into a firm jelly in the vessel beneath the table. The cyst, being completely emptied, was easily separated from everything, except at one spot where it adhered to the side of the lumbar spine. Here it appeared as if continuous with the periosteum of the vertebra. It seemed imprudent to use any force in tearing through this connection, as large vessels lay in the vicinity, and the region was not one within easy control. I therefore transfixed this portion of the cyst with a double carbolised silk ligature, and having secured it, cut away the tumour. This left as much of the cyst wall behind as made a hole about the size of a crown-piece in the tumour after removal. The patient progressed without a bad symptom for three weeks and, just as I was about to grant her permission to get up, she began to complain of pain in the left loin, to exhibit signs of fever and ultimately, if not to shiver, at least to experience chills. This state continued for many weeks, until at last I discovered deep fluctuation in the left loin. On 30th December, I dissected down upon this and evacuated a

large abscess to her great relief. Her recovery afterwards was steady, although the sinus remained open for some months; and she is now well and strong.

CASE XXX presented somewhat similar features at first sight. Mrs. H., aged 38, was admitted into the Western Infirmary on 10th October, 1886. She first observed a swelling of the abdomen in December, 1885; but as she was pregnant at the time, she did not think much of it. She had a miscarriage in January following, with great hæmorrhage, and, on her recovery, found that the swelling persisted. The report of her case is too long to be read here; but I find that on 10th October I dictated the following short note:—"A large nodulated tumour occupies the left of the abdominal cavity, but in no way apparently encroaches on the pelvis. On its inner side it hardly extends beyond the umbilicus and linea alba; above, it passes under cover of the ribs; while, on the outer side, it reaches backwards to the lumbar region, while patient lies on her back. It is freely movable from side to side, but cannot be pushed downwards towards the pelvis. It also falls readily from side to side according as the patient lies, but does not descend in the least when she is in the erect posture. It is nodulated and firm in character, but a distinct sense of fluctuation is felt over the whole of it, and more especially over the prominent nodules, giving the impression of a cavity or cavities tightly distended with fluid. It is somewhat tender upon firm pressure." On 20th October I aspirated the tumour, and drew off about 2 ounces of a dark viscid fluid. I immediately stopped the aspiration, as sufficient had been got for examination. Dr. Coats reported—"This fluid contains abundant cells, most of them fatty and many with complete fatty degeneration of the compound granular corpuscles. There are also abundant crystals of cholesterine. I should think it must be from an ovarian cystoma." On 25th October exploratory incision was determined upon. As in the last case, the omentum was in front of the tumour, and had to be pulled up. The fluid was viscid, and flowed with difficulty, while the puncture with the trocar bled freely. What appeared to be a closely adherent membranous expansion was with great toil and difficulty separated (it really was a second layer of peritoneum), and at last it was apparent that the cyst was so closely connected both with spleen and pancreas that it seemed impossible to remove it. The patient, who had been very faint throughout, was now very low. I therefore cut away all the detached cyst, and sewed the remainder to the wound in the

abdominal wall. The operation occupied about three hours. She never rallied, and died that night. Mr. Maylard examined the parts after death, and found that the cyst was really one of the pancreas, the whole of that organ, except its head, being converted into a huge cyst. I shall leave him to describe its character and relations in greater detail. I may be asked why I proceeded so far with this operation—why, in fact, I did not stop in time? I reply that it was a case of “*nulla vestigia retrorsum*.” I had gone so far before the desperate nature of the tumour’s connections was evident that my steps could not be retraced.

Of the remaining twenty-eight cases—all of ovariectomy—two were incomplete, of which one recovered and one died. The fatal case (xxviii) was that of a cyst so universally and closely adherent that I was compelled to give up the attempt to remove it before I had proceeded far. The other (case xxvii) was one of dermoid cyst, which had already become adherent to and formed a communication with the rectum. This woman had been twice an inmate of one of our infirmaries with a large fluctuating swelling in the lower part of the abdomen. It was easily felt in the vagina, and I was told, when I first saw her at her own house, that it had been aspirated, *per vaginam*, on each occasion of her being in hospital, and that large quantities of pus were removed. She suffered constant and severe pain, which at times amounted to excruciating agony, and was accompanied by much sickness. It was resolved that she should come under my care at the Training Home for Nurses, and that I should there endeavour to evacuate and drain the abscess, as it was supposed to be. Before I again saw her, however, a communication had been formed with the rectum, and considerable quantities of matter were said to be discharged *per anum*, now and again with great relief to the patient’s sufferings. After her admission to “The Home,” I had the opportunity of seeing a stool largely mixed with a yellow matter which appeared quite like pus. On 26th November, 1886, I made an incision through the middle line of the abdomen, and after opening the peritoneum, discovered a smooth and firm cyst occupying the left side of the abdomen, and very adherent to all around. In order to relax it, and admit of my drawing it up to the level of the abdominal wall, I aspirated a large portion of its contents. These were really not purulent, but were composed of a bright yellow thick fluid, which coagulated in the bottle into a greasy thick mass, like hair pomade. Considering the fact that the tumour was not only closely and generally adherent in the abdomen, but

had already formed a communication with the bowel (probably the rectum) it was thought well to do no more than stitch it to the abdominal wound, and then incise and drain it. This was accordingly done, a substantial drainage tube being introduced. From the interior of the cyst, coils of coarse black hair, much of it of considerable length were extracted, and a quantity of the same was found attached to the wall of the cyst, and was cut off short. The patient made a good recovery, and, although the fistulous opening persists, she was, when I last heard of her, very well. She has been free of pain since the operation. The remainder of my list includes twenty-six cases of ovariectomy. The ages of the patients varied from 19 to 69. The operations were of all degrees of severity. One of them was completed in less than half-an-hour, several others consumed two, two-and-a-half, and three hours in their performance. Three, possibly four, may be said to have been absolutely free from adhesion, while all the rest gave more or less trouble in this respect. In every case the pedicle was treated by ligature, and dropped into the peritoneum. In the first three cases antiseptic catgut was used for this purpose; in the rest carbolic silk (Chinese twist). With the exception of the first seven cases, I have inserted glass drainage tubes for 24 or 48 hours after all operations involving the tearing through or ligaturing of extensive adhesions, and in those where fluid from the cyst has escaped into the abdomen. The carbolic spray was used in all except the last four cases. Twenty-two recoveries took place and four deaths. The four patients who died were all cases of unusual severity, and the facts concerning them are as follows:—

CASE VII was a very anæmic, cachectic looking woman, 47 years of age, who was admitted into the Western Infirmary in March, 1883, on account of an abdominal tumour of extremely rapid growth. Indeed, it was thought by all who saw her that, in consideration of her great pallor, and the rapidity of growth of the tumour (she had only first noticed it 5 or 6 weeks before admission), the probabilities were strongly in favour of the malignant character of her disease.

A note in the journal, however, probably gives the true cause of her cachectic appearance. "Four years ago," it states, "she cut her left radial artery by accident and had it tied. A scar marks the site of the wound, and all pulsation is absent from the artery. She lost a great amount of blood, and attributes her present anæmic and pale condition to this

occurrence." The explanation of the rapid increase in the abdominal swelling was evident at the operation. It was to be found in the fact that two tumours were present, one of each ovary. They jammed each other tightly in the pelvis, and although there were no adhesions of consequence, the least traction upon either tumour caused it to tear, their structure being of a soft, jelly-like, colloid material. Each tumour was multilocular, and filled with a viscid, honey-like fluid. Wherever I grasped and pulled, my fingers slipped into the mass, and out of the holes so made, fluid flowed freely into the peritoneal cavity. With the exception of this mishap, the operation was satisfactorily completed. When I had sponged out the abdomen as completely and thoroughly as I could, I closed it by carbolised silk stitches. She died in a week's time of peritonitis, no doubt of septic origin, and it was the result of this case which made me resolve not completely to close the abdomen, but to introduce drainage tubes in all cases in which much blood or much ovarian fluid has escaped during operation into the peritoneal cavity. From what I have since seen I believe this woman's life might have been saved by drainage.

One other case of extremely rapid progress in the growth of the tumour has occurred to me, and as its cause was a very different one from that just narrated, it may be interesting to refer to it here.

CASE XV.—Mrs. M'C., aged 46, was admitted into the Western Infirmary, on 12th May, 1884, with a very large and tense tumour of the abdomen, which occasioned much pain. She first noticed it only  $2\frac{1}{2}$  months previously. She had had attacks of vomiting frequently, and evidently was, when admitted, suffering from a certain amount of peritonitis; with pain, occasional sickness, and persistently somewhat elevated temperature. Upon operating, I found the fluid of the cyst composed largely of blood, that which flowed through the cannula being as dark as claret, while in the bottom of the cyst there were pounds of clot. The cyst wall looked green and sloughy, and there was abundant exudation over the parietal peritoneum, and also numerous soft, recent vascular adhesions. The explanation of all this was found in the fact that the pedicle was as hard and round as a piece of whipcord, and with the same sort of twisted appearance; indeed, so tightly twisted was it, that it had caused an almost complete stoppage of the circulation. This had led to extensive intracystic hæmorrhage, and consequent rapid increase of the size

and tension of the tumour. The appearance of its walls showed further that they were on the verge of sloughing, and I cannot doubt the operation was undertaken just in time to avoid this serious occurrence. The patient made an excellent recovery.

CASE XII.—My second death was that of a thin, delicate, lady whom I saw first with Dr. Finlayson. She had never been strong; and in youth had suffered from caries of the right wrist, which was ankylosed and marked with the cicatrices of numerous sinuses. Her case appeared to Dr. Finlayson, Dr. Reid, and myself, to be a very suitable one for operation. This was undertaken at the Nurses' Home, on 5th November, 1883. The cyst was found to be universally and intimately adherent to the parietes, omentum, intestines, and uterus. I almost despaired at times of being able to remove it, but after about three hours' work I succeeded. She never fairly rallied, and died in 48 hours.

CASES XIX and XXII.—My third fatal case was that of an old lady on whom I performed a second ovariectomy, sixteen months after having removed the first tumour. When I first saw her she was an inmate of Gartnavel Asylum. She was 68 years of age, and had been affected with mental disorder for some time. She was now, in that respect, comparatively well, and would have been dismissed by Dr. Yellowlees, had it not been for the pain she was suffering from a large ovarian cyst. After consultation with Dr. Yellowlees, it was agreed that I should content myself with merely tapping the cyst, and giving her relief thereby, since it was thought her age and unstable nervous system were such as to render any severer procedure undesirable. Moreover, there had been numerous attacks of pain with sickness, which probably indicated extensive adhesions. The first effect of the tapping was to give great relief, but it was followed by thrombosis of the veins of the right lower limb, with the usual pain, swelling, and fever, which characterise "a white leg." This illness lasted for some weeks, at the end of which she was rather weaker, while the tumour was as large and more tense than ever.

After consultation with Dr. Reid, and with the full consent of her relatives, Dr. Yellowlees and I now resolved to recommend the patient to submit to ovariectomy. For this purpose we had her removed to the Nurses' Home, and on 18th December, 1884, performed the operation. It turned out one

of great severity, and occupied nearly three hours. The adhesions were very extensive to the parietes, omentum, and intestines, but were especially close and strong deep in the pelvis at the back of the uterus. The day turned out a very foggy one; and I believe it would have been almost impossible to accomplish this part of the operation, had Dr. Reid not taken the precaution to bring with him the electric light. With this on his forehead, he illuminated whatever spot I was working at, so that every point could be seen as clearly as in sunshine. Numbers of bands of adhesion between the intestines, and large quantities of loose, solid matter, not unlike boiled vermicelli, were left in various parts. It seemed hopeless to remove all. The old lady was almost pulseless, and the surface of the body was cold, so that my whole object was to complete the operation as quickly as possible, and get her to bed. This it was which led me to neglect the usual and very important practice of examining the remaining ovary before closing the abdominal wound. Had I done so, I have no doubt I would have found in it good cause for removal, and, if this had been done, she would no doubt have been now alive. She rallied slowly, but in the course of some hours thoroughly, from the state of collapse just described, and made an uninterrupted and excellent recovery, going home to Helensburgh in good health and spirits a month subsequent to the operation.

A year after this—namely, in December, 1885, I saw her in Helensburgh, with her medical attendant, suffering as much pain and tension as ever in the abdomen from the growth of a second cyst originating on the other side. She was naturally much dispirited, and suffered frequently from sickness and vomiting. The tumour, besides distending the abdomen, tightly filled the pelvis and bulged into the vagina, through which the uterus could not be felt. With some difficulty she was persuaded to come to town, and once more submit to operation. This was performed again at the Nurses' Home, with the assistance of Drs. Reid and Yellowlees and Mr. Maylard. The operation was quite as severe as on the first occasion, and occupied very much the same time. I had great trouble in separating the right ureter, which was engaged in bands of adhesion low down, and had become hydronephrotic above. So dense and close was the connection of the cyst to the uterine wall that it could not be separated, and I had to leave a piece of about the size of the palm of my hand in connection with that organ. Once more

the old lady rallied well from her state of collapse, and progressed as satisfactorily as any patient possibly could for six days. During this time she had no pain, and the surface of the abdomen was deeply concave. Her temperatures, night and morning, continued normal; she slept comfortably, read the newspapers, and was cheerful and happy. A dose of castor oil was administered about mid-day on 17th February—that is, four days after the operation. In this matter I made a departure from my usual practice of moving the bowels for the first time by enema. This I did, at the patient's urgent request however. The bowels moved freely five times before nine o'clock at night, and, as might be supposed, left her rather exhausted. A half-grain morphia suppository was administered with good effect, and the bowels gave no further trouble. She was very well the next day, and I removed the drainage tube. At 1 o'clock A.M. on 19th February, the sixth day after the operation, her temperature was normal, but about 3 o'clock she began to complain of pain over the left parotid. By 6 o'clock her temperature had risen to  $100.6^{\circ}$ , and at 10.30, when I saw her, it was  $102.6^{\circ}$ , and there was well marked parotitis, with œdema of the orbit, and intense pain and tenderness over the swelling. Warm applications soothed these symptoms, but the swelling continued until the whole of the left side of the face, including the half of the lips, as well as the left tonsil and soft palate and tongue were all greatly swollen. About 4 o'clock on the morning of 21st February, when the distension of these parts was at its height, she suddenly became comatose (probably from effusion within the cranium), and quickly died. I don't think I ever met with a greater disappointment in practice. Her recovery from the first operation, in spite of her advanced age, the excellent promise of a similar result which the progress of the first five days held out on this second occasion, and my knowledge of the fact that the result might have been different had I removed the second ovary when I first operated on her, all contributed to produce this feeling in my mind. To illustrate further the value to be derived from the practice of inspecting the state of the remaining ovary in every case of ovariectomy, I may refer to an operation performed by me (case xxiii) on the 11th of August last, on a patient sent to the Western Infirmary by Dr. McKinlay, of Barrhead. In this case I found the remaining ovary to be in a condition of cystic degeneration, one of the cysts being about the size of a marble. Its removal produced no effect on the patient's progress towards recovery, except that which seems invariable



where both ovaries are removed—namely, the occurrence of a menstrual discharge within forty-eight hours of operation, an occurrence never to be repeated in the individual's life. Parotitis, as a sequela of wounds of and operations on the abdomen, is an interesting clinical occurrence.\* In these, as in other surgical cases, inflammation and abscess of the parotid gland occur now and again, as an incident in a general pyæmic attack, but in my patient's case just narrated, the parotitis came like a bolt out of a clear sky; and up to the time of death her abdomen was free from all pain or tenderness on pressure, while, so far from being distended, it was, as has been stated, deeply concave.

CASE XXVI.—My last and fourth fatal case was one of unusual interest. It occurred in the person of an unmarried lady over forty years of age. In the spring of last year she felt herself not quite so vigorous as usual, and became conscious that she was growing stout, having experienced the necessity of "letting out" her garments. In June, she went on a visit to friends in the highlands of Morayshire, with the view of improving her health by change of air, and while there, one day, met with a severe accident. When in the act of sitting down on the back seat of a dog-cart, the horse moved forward and she fell out prone. She struck her abdomen with great violence on the road, and was at once seized with severe pain and vomiting. The belly began to swell rapidly, and ill as she was, the doctor in attendance thought she ought to return as fast as possible to her home in Rutherglen. On the 11th July, Dr. Finlayson saw her there in consultation with Dr. Goran. She had an enlarged and tense abdomen, rapidly increasing in size, so that she could scarcely ever lie down. The breathing was much distressed. On examination the abdomen gave signs of the presence of fluid, which was distinctly movable on changes of posture, and was therefore regarded as being in the peritoneal cavity. She was not very feverish. Search was made for the presence of tumour, as, on account of the previous failure of health without any known abdominal tumour, suspicion of malignancy was entertained, the injury being thought to have given it, perhaps, a fresh start. No tumour was discovered, and some palliative treatment was prescribed. In less than a week, however, on account of increasing distress, she was tapped with a syphon

\* Since the reading of this paper, the subject of parotitis, following abdominal wounds and operations, has been treated at length by Mr. Stephen Paget, in the pages of the *British Medical Journal*.

tube. Attention was at once directed to the character of the fluid, which seemed characteristic of ovarian origin—dark coloured, sticky, and glittering with cholesterine crystals. A solid mass in the left iliac region remained after the withdrawal of the fluid, and was taken to be a collapsed and ruptured ovarian tumour. The patient was relieved, but pain came on soon after in the abdomen, and next day pleuritic friction was heard by Dr. Gorman, from which she recovered in about a week. The belly again enlarged, but not rapidly, and not with the same great respiratory distress. The physical signs were now also those of encysted fluid, and not, as before, of fluid free in the peritoneum. It was evident that the rupture in the wall of the cyst had closed, and the tumour was once more being filled up. The swelling gradually increased, and pain at last became severe and pretty constant. Dr. Reid and I examined her more than once with Dr. Finlayson and Dr. Gorman, and in the beginning of November her state had become one of urgency. Her sufferings were great, her breathing so distressing that she sat day and night in a chair, and in consequence her legs and feet were enormously cedematous, and at parts red and inflamed. I operated on 12th November last. The operation was most severe, the adhesions being intimate and general, both in the abdomen and pelvis. Their separation and the staunching of the consequent hæmorrhage occupied a long time, and altogether the operation lasted about three hours. She rallied fairly well, and progressed satisfactorily for a day, when symptoms, which I took to be those of peritonitis, supervened. Her state was characterised by restlessness and vomiting, with a quick pulse and a subnormal temperature. She died on 15th November, three days after operation.

So much for my fatal cases. I will not weary the Society by reading the tedious details of those which recovered, but I may be allowed, in conclusion, to refer to certain interesting complications which occurred in the course of some of them. Last session I had the honour of reading a paper before this Society in regard to hæmorrhages and congestions occurring as the result of the sudden withdrawal of some counter-pressure which has long been acting on the vessels of the part, and this principle has, I think, been at work in producing some complications which I have observed. These are principally sudden pleuritic effusions, pulmonary congestions, hæmorrhages, and profuse watery evacuations from the bowels. I shall, as shortly as possible, detail instances of these.

CASE XIV was that of an old unmarried lady who was within a year of completing the "threescore years and ten." I had examined and tapped her tumour, at her own house, in the beginning of 1884. It refilled, and in the spring of the year was causing her great distress. After a consultation with Dr. Renfrew and Dr. Reid, it was resolved that she should go to the Nurses' Home and submit to ovariectomy, which operation I performed, with the aid of those gentlemen, on 16th May, 1884. She was a thin and feeble looking old woman, and the operation, on account of adhesions, was rather a severe one. She rallied well, however, and progressed satisfactorily up till the evening of the following day, when a troublesome cough set in, with some pain in the right side, restlessness, and increased temperature. By the next day the physical signs indicated the existence of a large quantity of fluid in the right side of the chest. For ten days she continued in a state which caused considerable anxiety, but without any very active treatment her condition began to improve, the fluid absorbed, the cough lessened, and she ultimately made an excellent recovery. Another interesting fact may be mentioned in reference to this case. Three months after her recovery I was asked to see her sister, who was a year her junior, and whom Dr. Renfrew and I found on examination to be also affected with a large ovarian tumour. At the time of her sister's operation she had concealed the fact from us, and watched her progress with a double interest in her fate. She was admitted to the Home in September, 1884, and I removed her tumour on the 29th of that month. It was multilocular, and in great part semi-solid. There were considerable parietal and omental adhesions, while the solid mass filled the pelvis completely. It required considerable traction to draw it out, but there were, fortunately, no deep adhesions, and it came away with a loud sucking noise. Like her sister, she made an excellent recovery. It is interesting to think of these two women, both unmarried, living together all their lives, and having this experience in common towards their close. They lived alone and had never been separated. Like Celia and Rosalind, "they had slept together, rose at an instant, learned, played, sat together; and wheresoe'er they went, like Juno's swans, still they went coupled and inseparable." It is a curious instance of how close a resemblance there may be in the episodes of the health history of lives which have had a common parentage and similar habits and experience.

To revert to the subject of the pleuritic effusion, I think that the occurrence, as I have related it, may fairly be attributed to the sudden removal of the distention of the abdomen. One can hardly estimate the great change that must take place in the circulation within the thorax after such an operation; and it was to myself no matter of surprise that in this old woman, without pain or other premonitory symptoms, extensive effusion should take place then. The occurrence of a pleurisy after tapping, in Case xxvi, just related, was evidently an analogous occurrence.

CASE VIII was, I believe, an example of much the same sort of thing. It was, however, characterised by pulmonary consolidation rather than by effusion. The patient's chest was so far unsound that she had a slight habitual cough, aggravated when she caught cold. Her cough began to trouble her shortly after the operation (21st September, 1883), and on the fourth day she complained of a stitch in her left side, while fever set in, with restlessness and shortness of breath. Dr. Samson Gemmell saw her next day. It is stated in the Ward Journal that he found considerable consolidation over the greater part of the left lung. He prescribed for her, and continued to visit her daily. In ten days she was convalescent, and made subsequently a very good, although slow recovery.

CASE I illustrates also, I believe, an evil result of the sudden withdrawal of support and counter-pressure to the abdominal vessels. The following account of the case is extracted from the Ward Journal:—"Eighteen months ago her abdomen was tapped by a medical man in the country, and again in June last" (she was admitted to Hospital on 13th September), "and very large quantities of fluid were drawn off on each occasion. She is pale, and somewhat thin, but states that she is in fair health. There is a large oval tumour, tense, and of a uniform consistence throughout, occupying the whole abdomen. A distinct wave of fluctuation can be communicated from one hand to another from almost any two points. Clear percussion is distinct in either lumbar region, while all the rest of the abdomen is uniformly dull." On 20th September I withdrew some fluid from the tumour with cannula and trochar for diagnostic purposes. After this tapping (several pints) the patient became pale and alarmingly faint, and complained much of severe pain in the epigastrium. In the course of half an hour or so

she vomited a quantity of blood, and her pain ceased. She remained faint and ill, however, till evening, when she rallied, and was quite well again next day. She told us that exactly the same thing happened when she was tapped in June last. On 12th October I removed the tumour, which was partly solid, but consisted in the main of a single large cyst. On being removed to bed patient seemed very well, but shortly passed into a faint, almost collapsed condition, which lasted about an hour. Her temperature was subnormal, and her pulse very thready. This condition was treated by raising the foot of the bed and administering stimulants. She also complained of severe pain, principally in the epigastrium, but this was much relieved by a  $\frac{1}{2}$  grain morphia suppository. Three or four hours afterwards she vomited a quantity of altered blood of a "coffee ground" colour. After this she quickly rallied, and left the Hospital on the 29th November perfectly well. She has continued in good health ever since.

The interesting feature of this case is the alarming syncope and collapse which thus occurred after two tappings, and also after removal of the cyst. On all of these occasions there was great pain over the stomach, followed by hæmatemesis. There can be little doubt that these symptoms occurred as a consequence of the sudden relief of pressure by the withdrawal of fluid, and the consequent alteration in the circulation of the stomach. The occurrence seems comparable with those grave mishaps which sometimes follow the removal of large pleural effusions by paracentesis. Sudden death occasionally happens, presumably from the circulation being restored to portions of the lungs where it has long ceased, and so even disturbing thrombi with fatal effect.

The only late bad result which I know to have followed upon any of my operations is the occurrence in two cases of ventral hernia through the cicatrix. Both of the women who so suffered are, from necessity, hard workers, and in both the incision required for the safe conduct of the operation was a very long one. Indeed, in one of these cases, a large semi-solid multilocular tumour of many years' standing, I was forced to incise almost from sternum to pubes. No fluid would flow through the cannula, and although I buried my hand and forearm in the tumour and broke it up as well as I could, so rigid and unyielding were its walls that I could only reach the adhesions and turn out the mass by freely extending my incision. One day, some months after her recovery, she was shutting a heavy window, when she felt something give way

in the scar, and on examination she discovered a swelling. The rupture is one that might be easily repaired; but she declines in the meantime to have it interfered with, preferring to use a support made for her by Mr. Hilliard.

### THIRTY CASES OF ABDOMINAL SECTION.

#### COMPLETE OVARIOTOMIES.

| No. | Name.         | Age. | Where Treated.     | Date.          | Result.    |
|-----|---------------|------|--------------------|----------------|------------|
| 1   | I. K., .      | 28   | Western Infirmary. | Oct. 12, 1881  | Recovered. |
| 2   | Mrs. R., .    | 30   | Do.                | Dec. 17, 1881  | Do.        |
| 3   | A. M'K., .    | 19   | Do.                | Mar. 6, 1882   | Do.        |
| 4   | E. D., .      | 24   | Do.                | June 26, 1882  | Do.        |
| 5   | M. C., .      | 58   | Do.                | July 4, 1882   | Do.        |
| 6   | Mrs. M'N., .  | 45   | Do.                | Jan. 22, 1883  | Do.        |
| 7   | Mrs. G., .    | 47   | Do.                | Mar. 28, 1883  | Died.      |
| 8   | Mrs. A., .    | 38   | Do.                | Sept. 21, 1883 | Recovered. |
| 9   | Mrs. M'G., .  | 50   | Nurses' Home.      | Oct. 15, 1883  | Do.        |
| 10  | E. M'A., .    | 18   | Western Infirmary. | Oct. 12, 1883  | Do.        |
| 11  | A. M'M., .    | 30   | Nurses' Home.      | Oct. 16, 1883  | Do.        |
| 12  | Mrs. T., .    | 35   | Do.                | Nov. 5, 1883   | Died.      |
| 13  | S. B., .      | 53   | Western Infirmary. | Apr. 26, 1884  | Recovered. |
| 14  | Miss C., .    | 69   | Nurses' Home.      | May 16, 1884   | Do.        |
| 15  | Mrs. M'C., .  | 46   | Western Infirmary. | May 23, 1884   | Do.        |
| 16  | Mrs. B., .    | 38   | Nurses' Home.      | May 24, 1884   | Do.        |
| 17* | Miss M. C., . | 68   | Do.                | Sept. 29, 1884 | Do.        |
| 18  | M. M'M., .    | 22   | Do.                | Nov. 19, 1884  | Do.        |
| 19  | Mrs. D., .    | 68   | Do.                | Dec. 18, 1884  | Do.        |
| 20  | Mrs. M., .    | 39   | Western Infirmary. | June 3, 1885   | Do.        |
| 21  | Mrs. C., .    | 40   | Nurses' Home.      | Oct. 22, 1885  | Do.        |
| 22† | Mrs. D., .    | 69   | Do.                | Feb. 13, 1886  | Died.      |
| 23  | Mrs. L., .    | 54   | Western Infirmary. | Aug. 11, 1886  | Recovered. |
| 24  | A. M'M., .    | 42   | Do.                | Aug. 24, 1886  | Do.        |
| 25  | Mrs. H., .    | 42   | Do.                | Sept. 20, 1886 | Do.        |
| 26  | Miss S., .    | 46   | Nurses' Home.      | Nov. 12, 1886  | Died.      |

#### INCOMPLETE OVARIOTOMIES.

|    |            |    |                    |               |            |
|----|------------|----|--------------------|---------------|------------|
| 27 | Mrs. S., . | 38 | Nurses' Home.      | Nov. 26, 1886 | Recovered. |
| 28 | Mrs. M., . | 34 | Western Infirmary. | July 21, 1885 | Died.      |

#### CYSTS NOT OVARIAN.

|    |            |    |                    |               |            |
|----|------------|----|--------------------|---------------|------------|
| 29 | Mrs. K., . | 42 | Nurses' Home.      | Oct. 25, 1884 | Recovered. |
| 30 | Mrs. H., . | 39 | Western Infirmary. | Oct. 25, 1886 | Died.      |

\* Sister of Case 14.

† Same patient as Case 19; second operation.

NOTES ON THE TRADITIONS AND CUSTOMS OF  
THE NATIVES OF FIJI IN RELATION TO CON-  
CEPTION, PREGNANCY, AND PARTURITION.

By DAVID BLYTH, M.B., and C.M.,  
Late of the Government Medical Staff, Fiji.

THE Fiji group of islands, situated within the tropical area of the South Pacific Ocean, is the abode of a superior native people, interesting alike to the ethnologist and the student of folk-lore. The Fijian race forms a connecting link between the Malayan and Papuan inhabitants of the widespread Polynesia; and historically the Fijians present many features which distinguish them from the other numerous aboriginal tribes which inhabit the islands collectively constituting the Polynesian archipelago. From time immemorial the Fijians have had their "medicine men" and midwives, to whom the sick inhabitants of the village and parturient females have invariably been confided. The doctor of medicine was called in the native language the "vuni wai," or "root of medicine," and the midwife the "alewa vuku," or "wise woman." These individuals, in the practice of their respective arts, took care to surround their actions with a mystery which no one dared to penetrate, and the results of these actions no one ever thought of discussing. The profound secrecy with which the healing art was invested lent a dignity to its professors, who were regarded with reverence by the unsophisticated people among whom they practised. The Fijian doctor and midwife were under an obligation to preserve inviolate the mysticism of their craft during their lifetime, and when from age, or any other cause, they ceased to practise, the secrets of their calling were carefully communicated to those who had been chosen as their successors. In this manner the system of medicine of the vuni wai, and the mysterious handicraft of the alewa vuku, were transmitted from generation to generation. No pupils were initiated during the lifetime of the doctor or midwife, or rather while they continued to practise their calling; and nothing short of retirement from work could induce them to communicate their mysteries to another person, that other person being a successor.

The Fiji Islands have now, for twelve years, formed an integral portion of the British Empire, and, as in all cases where civilisation exerts its influence upon an inferior people, the racial characteristics of the latter give way before the

institutions of the former, so the traditions and customs of the Fijians are gradually, though surely, being supplanted by European customs and practices, though perhaps not with unqualified advantage to the native race. To-day, therefore, though the Fijian midwife still practises her craft unmolested among her native tribes, and in remote districts extends her aid to European women as well, the doctor, or vuni wai, has to a large extent ceased to monopolise the healing art among the Fijians. In other words, the Fijian now largely avails himself of the benefits of European medicine as practised by the government medical officers scattered throughout the group. Indeed, a system has been inaugurated during the last three years by the Hon. Dr. Wm. McGregor, the able chief medical officer of the colony, by which intelligent and selected Fijian youths are being instructed in the elements of medicine at the Government Hospitals, and who, after serving their probationary period, are drafted off to various parts of the colony, for the purpose of treating the minor ailments of the natives, and acting as vaccinators amongst them.

The followings notes on the traditions and customs of the Fijians as bearing on sexual reproduction, jotted down during my residence as Government Medical Officer in the group, may possess a certain amount of interest. The midwife, even to this day, preserves the secrecy of her art, and it was solely in my capacity as a medical man that the facts embodied in the following notes were, in the course of occasional conversations, confided to me :—

As in all tropical countries, so in Fiji, puberty occurs at an early age, Fijian girls beginning to menstruate at ten years on an average. The advent of puberty is looked upon as an indication of the cessation of growth. Cases of delayed menstruation, corresponding to the *Emansio Mensium* of European practice, are not unknown amongst the Fijians, girls growing up into womanhood, and even entering the married state without ever having menstruated. A midwife informed me that in such a case, where a woman marries without having previously menstruated, the first coitus is invariably followed by a disturbance of the system far more serious and protracted than takes place when the menstrual functions have been duly established. In these cases of delayed appearance of the menses no attempt is made to remedy the functional quiescence, it being deemed more judicious to trust to Nature than to interfere.

Menstrual irregularities are not unknown among the



women of Fiji, and this is not surprising, inasmuch as they are guilty of gross indiscretions, such as bathing in streams, and wading in the sea for fishing purposes, during the menstrual period. For suppression of the menses a remedy prepared from the bark of the Vesi Ndina (a tree of the *greenheart* species) is employed, the bark being scraped and an infusion made from the scrapings. This succeeds in some cases I was told, and, if it fail, nothing else is of avail. The midwives affirm that Fijian women have been known to die from suppression of the menstrual discharge, but this statement probably in reality means that diseases attended with, or giving rise to cessation of the menses, occur in Fiji.

Menstruation is sometimes accompanied with pain in native women, such cases being comparable, in a general sense, to dysmenorrhœa in European practice. When a Fijian woman is seized with pain during a menstrual period, a midwife is sent for, who at once, on her arrival at the abode of the suffering women, proceeds to prepare an infusion for the patient. In this case the stem and leaves of a vine—the *Wa Ndamu*—are used. A segment of the stem is first denuded of bark, and then the outer sappy wood is scraped. The scrapings, together with the leaves of the creeper, are made into an infusion with cold water, and a dose immediately given to the sufferer. Food is now prepared for the midwife in return for her professional services, and having partaken of the repast provided, she departs, giving injunctions that if the woman is not completely well in four days (a very safe allowance of time, certainly) she (the midwife) is to be summoned again. Should the midwife have occasion to return, she administers another dose, food being liberally prepared for her as before. This herbal preparation is looked on by the native "Faculty" in Fiji as an infallible specific for painful menstruation—or "*Dravutu*," as it is called—the second dose being invariably successful should the first one fail. No other measures are adopted besides administering this herbal remedy.

Fijian women, taken as a class, are by no means prolific, although individual females may give birth to ten, eleven, or even twelve children, during the child-bearing period. Sterile marriages, I have been informed, are of frequent occurrence among the race. This statement at first surprised me, and I was inclined to doubt its accuracy, but on making enquiries from various reliable sources, I found it to be a fact that sterility is by no means uncommon among the Fijians. A

sterile marriage is, as a rule, attributed to a defect on the part of the woman, although I have been told that on several occasions impotency has been believed to exist in male Fijians, who failed to beget children by particular females. If a woman is sterile, she is at once believed to have drunk at some time or other "the waters of barrenness," that is to say, sterility is looked upon as a self-induced condition. In cases of sterility a medicine is given to the woman with the intention of inducing fecundity. The remedy so employed is procured from two sources, viz.—(1) the Mbokase, a kind of bread-fruit, and (2) from the Rerega, or Cago (pronounced Thango) a species of turmeric. Of the former plant the scraped root, and of the latter a nut, or kind of fruit is used, and from these a decoction is as usual prepared. The remarkable and interesting fact in connection with this treatment is that both the male and female have to drink the decoction. Previously, however, the woman bathes in a stream, then both she and the husband drink the potion, and immediately afterwards coitus is supposed to take place.

I have myself heard a Fijian midwife affirm that she has known the foregoing remedy successful in curing sterility in three cases, children afterwards being born of the marriages. As I was only making a collection of traditions and folk-lore, I accepted the statement implicitly! In one of these three cases the success of the midwife was so astonishing to the woman she had cured, that she at once bought the recipe from the former, giving, as its marketable value, a large amount of property in the shape of native cloth, or "tappa," whales' teeth, mats, cocoa-nut oil, and other Fijian commodities. This was clearly a breach of professional trust on the midwife's part, but the Fijian conscience is elastic!

When a Fijian man and woman are married they remain in strict seclusion for three days; on the fourth day the women of the same town assemble and escort the newly-married female to a stream to bathe, and her husband is now supposed to abstain for a lengthened period from sexual indulgence. This custom, however, properly speaking, belongs to the time when the Fijians were polygamists, and paid rigid regard to the domestic and tribal laws imposed upon them, a regard which was deepened by a knowledge of the fact that an ignominious death by the club awaited those who violated the rules of the household or the tribe. By and bye missionary influence established monogamy amongst them; and, nowadays, as civilisation advances in their midst, promiscuous sexual intercourse becomes the rule, and the golden

maxims, which in former times regulated marital relations, and undoubtedly conduced to the health of both parents and offspring, are ignored and forgotten.

Just as the Fijian midwife undertakes to rectify sterility, so, on the other hand, amusing expedients are resorted to with the object of preventing conception, and these methods are believed to be sometimes successful and sometimes not. The medicine employed for this purpose is obtained from the leaves and root of the Roqa tree, and from the leaves and root of the Samalo in conjunction. The roots are first denuded of bark and then scraped. The scrapings and the leaves bruised are made into an infusion with cold water, and this, when strained, is ready for use. This herbal medicine is taken sometimes once, and sometimes twice, in order to produce the desired effect. If coitus take place, say in the evening, the decoction is given on the following day, and this without any reference to the relation in point of time which the coitus may bear to the menstrual period. This remedy, besides being given to prevent a first conception, is also administered in the case of a woman who has had one or more children, in order to prevent future pregnancies.

As might be expected, pregnancy among Fijian women is not attended with the usual concomitants met with in European females. Menstruation in many instances persists uninterruptedly during utero-gestation. Fijian women, when pregnant, never suffer from "morning sickness," but are liable to attacks of vomiting at mid-day. Throughout their pregnancy they are subject to giddiness, and are, from that cause, liable to fall suddenly to the ground. This giddiness and sudden falling are so common amongst them that they are regarded as characteristic signs of pregnancy. Hence, if a Fijian woman is seen to fall unexpectedly to the ground, the villagers at once remark—"she is pregnant." Fijian females suffer from no other disorders in connection with gestation. Quickening, the native midwife affirms, takes place two months after the cessation of the menses, but as the Fijians have very imperfect ideas regarding the measurement of time, and make all their calculations by counting successive "moons," I did not place any reliance upon this statement. On asking the duration of pregnancy, however, the reply was that it lasts ten moons—that is, ten lunar months.

In former times, when polygamy was a recognised institution, there existed a hard and fast domestic rule that immediately on a wife becoming pregnant, cohabitation between her and the husband common to the band of wives, should

cease until the progeny of that particular pregnancy had attained the age of two years; in other words, until the child had passed through the period of lactation. This separation *ab thoro* during pregnancy and nursing was, in bygone times, invariably enforced, but in these days the rule is disregarded. Now, as formerly, however, it is a general belief among the Fijians that the children born of a marriage are most vigorous and healthy when the husband and wife rarely cohabit; and if one or more members of the family should turn out to be weak or unhealthy, the mother at once attributes the physical disability of her offspring to sexual excesses on the part of the father.

The Fijians regard one coitus as inadequate to induce conception. A curious superstition among them is that if a young unmarried man has one illicit connection and does not repeat it, he is sure, sooner or later, to be seized with some wasting disease, and ultimately die! Hence, though his conduct was wrong in the first instance, he is advised—in fact, compelled—to renew the cohabitation, in order to prevent his falling a victim to the inevitable malady.

We come now to an interesting subject—one, in fact, which has engaged the attention of many thoughtful persons—viz., the practice which has at all times prevailed extensively among the Fijians of procuring abortion. About eight or nine years ago the mortality among the native population was so high that a Commission was appointed by the Governor of the colony to enquire into the causes of the excessive death-rate. After careful scrutiny, it was found that the heavy mortality depended on two causes—first, on the induction of abortion as affecting the female population; and, second, on dysentery as affecting the natives generally.

I have heard a Fijian midwife positively assert that a spontaneous miscarriage is totally unknown among Fijian women, and that when an abortion takes place it is invariably looked upon as induced, and not accidental. This practice of artificially terminating utero-gestation seems to have had its origin in several ways. Fijian women have a decided aversion to large families, and have a feeling of shame if they become pregnant too often, believing that those women who bear a large number of children are laughing-stocks to the community. Hence it would appear that Fijian women often induce abortion with the object of curtailing the number of their offspring; or if a woman believes that her present pregnancy has too quickly succeeded her previous one, she deems it necessary to bring about abortion. Again, native women are

said frequently to induce miscarriage in order to spite their husbands when by the infidelity of the latter jealousy has been provoked. Further, in cases of illegitimacy the woman resorts to the practice in order to hide her shame.

As to the methods adopted for the purpose of inducing abortion, the Fijians positively affirm that they employ no other means whatever than the administration of herbal decoctions, these being given when the woman first experiences quickening. Although this assertion regarding the mode of bringing about miscarriage has many adherents among the European residents in Fiji, I have personally always refused to believe it, and am quite convinced that besides giving simple herbal infusions, it is the usual practice to initiate uterine action by rupturing the membranes, or in some way interfering with the attachments of the ovum to the uterine walls. It may be interesting to note here the plants used by the Fijians in procuring abortion. They are, according to the late Dr. Berthold Seemann, five in number, as follows:—Two from the natural order *Malvaceæ*—viz., the *Kalakalauaisoni*, or *Hibiscus diversifolius*, a spiny shrub growing in swamps, and the *Wakiwaki*, or *Hibiscus abelmoschus*, bearing large yellow flowers, destitute of spines, and invariably preferring dry soil; one from *Tiliaceæ*, the *Siti*, or *Grewia prunifolia*, a small tree abounding in the group, and producing a fruit eaten by the Fijian bat; one from *Convolvulaceæ*, the *Wa Wuti*, or *Pharbitis insularis*, a seaside creeper, bearing flowers, blue in the morning and turning purple towards sunset; and, lastly, one from the *Liliaceæ*, the *Ti kula*, or *Dracæna ferrea*. Of the first, second, and fourth, the juice of the leaves, and of the third and fifth the juice of the leaves and surface of the trunk are employed. The last mentioned in the above list—the *Ti kula*, is regarded as the most efficacious, and is only administered when the others have failed in producing abortion.

Such are the plants used by the Fijians for the purpose indicated. But when we consider that there is hardly a drug in our pharmacopœia which, unaided, is capable of causing the uterus to expel its contents, it is very difficult to believe that with such simple agents the Fijians are able to accomplish the task.

Turning now to natural labour, we find in the Fijian race an exemplification of the law that parturition becomes more and more an artificial process as we ascend in the scale of organisation. As a rule, then, parturition is speedy and easy among Fijian women, although tardy and difficult labours

are not entirely unknown. Labour, as a general rule, lasts, on an average, about six hours or so; that is to say, if the pains set in at mid-day the labour will be completed at sunset, and if it begin at sunset it will be finished by midnight. The woman, it may be mentioned, assumes the prone, recumbent position. If the birth of the child does not speedily follow the bursting of the membranes, the midwife inserts her fingers into the ears of the child and pulls, or she pushes against the shoulders of the woman in order to expedite the labour, while the midwife at the same time keeps saying to her, "exert yourself;" "assist us." I think we may safely conclude that the midwife's exhortations to the woman are more consistent with common sense, and savour less of tradition than the expedients above described—viz., pulling by the ears of the foetus and pushing against the shoulders of the woman. No preparation, so far as I could discover, corresponding to ergot in its application, is ever administered to native women with the object of facilitating labour. The child born, the navel-string is never cut until the placenta has been expelled, which it usually is along with, or immediately after, the child. Should there be a tardy expulsion of the after-birth, the umbilical cord is tied to the woman's thigh to prevent the placenta from moving farther upwards out of reach! The midwife now introduces her hand right into the womb, and removes the after-birth. If she has any difficulty in doing so, she at once declares it to be adherent to the uterine walls, and proceeds to give the woman an infusion prepared from the *Ndanindani* plant, which grows luxuriantly in Fiji. This decoction is intended to effect the separation of the placenta, and in a few minutes the "wise woman" again introduces the hand and removes it from the womb. There is here no idea of forcible separation of the placenta by the hand, and doubtless what the Fijian midwife regards as an instance of what we would call "adhesion," is simply a case of retention, or possibly of tardy separation from the uterine surface.

The after-birth being expelled or removed, the navel-string is now cut with a mussel shell. The foetal stump is never tied, but is merely wrapped loosely in a piece of native cloth. Bleeding sometimes takes place from the untied umbilical end of the cord, but no efforts are made to staunch this. The midwife simply trusts to Nature's hæmostatics, and no fatal results ever accrue from the hæmorrhage.

In the event of *post-partum* bleeding—and in some cases it is profuse—the clots are removed from the vagina and the os,

and the woman is then immediately conveyed to a stream, where she bathes, and has the water laved against the external parts. If the woman is too weak to be removed to the brook, the process is carried out in the house. This application of cold water is in some cases adopted at intervals for four days after delivery, and, I was informed, always succeeds in arresting the hæmorrhage. The midwife from whom I got this information never knew a woman die from *post-partum* flooding, and I was highly amused to hear her say that the more women bleed after delivery the better, as they are then lighter (!) and stronger afterwards than they could otherwise be.

After the birth, the child is removed completely from the mother until milk is secreted in the breasts, and, as a rule, the mammæ contain abundance of milk on the second day after delivery. It may be delayed, however, for four, five, six, or even for as long as ten days, and in such cases the leaves of the *Lolo ndamu*—the red-leaved fig—are applied to the breasts with the intention of inducing the lacteal secretion, the leaves being previously warmed. In the absence of the mother's milk the infant is meantime fed on the milk of the cocoa-nut. Fijian women continue to nurse their children until they are two, and in some cases three years of age. At first I was disposed to doubt this statement, but I found that it is an absolute fact that Fijian mothers prolong lactation to that extent. It is the usual custom, however, to give infants the *ndala*, or *taro* (*Arum esculentum*) and the young cocoa-nut along with the mother's milk, even in the earliest months of their existence; feeding and suckling go on together in fact.

The breasts of Fijian women who have nursed children are remarkably pendulous, the true mammary gland being contained, as it were, in the extremity of a long *cul-de-sac* of stretched integument. Those mothers who possess such flaccid breasts are in the habit of throwing them over the shoulders when suckling, the child being perched on the mother's back.

In former times, women of high rank, such as the wives of the late King Thacombau, or of the chiefs of Fiji, never suckled their offspring at all, but gave their children to women of inferior blood to be nursed by them. But in these Christian days, even females of the highest social status are beginning to nurse their own children. Fijian women exhibit sound common sense as regards the period they allow to intervene between their confinements and the resumption of their ordinary duties, for they abstain from household work for at least ten days. The parturient woman bathes in the house on

the day succeeding delivery, and also on the second and third, but on the fourth day, and regularly afterwards, she proceeds to the stream to bathe.

Except in high rank, the Fijians regard the birth of a female child with great indifference, while the arrival of a male occasions no end of fuss, and is celebrated as a great event amongst them.

There are really no diseases incidental to the puerperal state in Fijian women, the only untoward circumstance of any importance that is liable to occur being a cessation of the lochial discharge about one or two days after delivery. This gives rise to a fit of shivering, which is succeeded by feverishness, headache, thirst, and other symptoms similar to those met with in European women from the same cause, while a sensation is experienced as if (to use a Fijian midwife's expression) "an orange were rolling about in the stomach." This subjective sign may presumably be caused by the pent up lochia in the uterus. When arrest of the lochial flow takes place, the native midwife is immediately summoned, and the treatment she adopts in the circumstances is certainly practical, and presents a remarkable similarity to the measures we employ in European practice. The midwife at once orders one or two fires to be kindled close to where the woman is lying, and assiduously applies hot banana leaves to the abdomen until the lochial discharge is re-established, just as we apply hot fomentations and stupes. As a rule, the lochial flow continues for ten days.

On the other hand, should a puerperal woman complain simply of pains in the abdomen, without any lochial arrest, or obvious disturbance, the midwife unceremoniously conducts her patient to the nearest stream to bathe, a method of treatment which to us appears at least injudicious.

Twin births are not uncommon among the Fijians, but a case of triplets has never been known. Presentations are almost invariably those of the head; a midwife with whom I conversed informed me that she had never, in all her experience (and, considering her age, it must have been extensive) seen any other than head presentations, although she had heard others talk of presentations of the feet. Breech cases do occur now and again, however, and in some of these the women die undelivered from want of assistance. This statement rests on the authority, not of a Fijian, but of a European missionary who had resided fifteen years in Fiji. He was intimately acquainted with the inner domestic life of the natives, and told me that instances of breech birth had come



within his personal knowledge in which the mothers had died undelivered. Such cases are certainly extremely rare, although there were no means of ascertaining their relative frequency.

During my three years' residence in Fiji I was never called upon to render assistance to a Fijian woman in labour. In one instance I applied the forceps in a case of a young girl, a native of the Solomon group of islands, the inhabitants of which are still savages, and vastly inferior to the Fijians in point of physique and intelligence. In this girl's case the presentation was of the head, and the application of the forceps was rendered necessary by the disproportion between the foetus and the maternal parts. Had the case occurred on this girl's native island she must inevitably have died from exhaustion, sloughing of the passages and its concomitants, with the head engaged in the pelvic cavity. In one other instance I was summoned to render assistance to a half-caste female whose case had been too much for the Fijian midwife. The husband, a half-caste also, informed me that the midwife had told him that the child was not properly placed. Suspecting the nature of the case I took chloroform with me. On arriving at the native hut I found what I had expected—namely, a transverse presentation. The poor woman had been in labour for twelve hours previously; the pains were still fairly regular and strong; the liquor amnii was entirely drained away; an arm, swollen and with epidermis peeling off, protruded from the vulva; while the maternal parts were terribly swollen and livid. Having put her under the influence of the anæsthetic, I returned the limb, and performed podalic version with very little difficulty, the placenta being expelled very soon afterwards. The child had been dead for some hours before I interfered. This woman made a surprising recovery, for on visiting the house on the fourth day after the event I found her attending to her household duties. It was interesting to watch the astonishment and eager attention of the midwife and her Fijian coadjutors, and to listen to their exclamations of surprise, while the woman whose miseries they had stoically beheld for hours inhaled the chloroform (or "water of sleep," as they call it), and the child, that had perversely insisted on coming the wrong way, presently changed its position under the manipulations of him whom they designated the "matai ni mate," or "carpenter of death" (surely not always an appropriate professional title), and whom, under such circumstances, they were inclined to regard as something more than human.

# CASE OF CEREBRAL ABSCESS DUE TO EAR DISEASE; LONG COURSE AND REMARKABLE INTERMISSION OF SYMPTOMS.

By THOMAS BARR, M.D.,

Surgeon to Glasgow Ear Hospital; Lecturer on Aural Surgery, Anderson's College, &c.

(*Read before the Glasgow Pathological and Clinical Society.*)

A. B., a girl 22½ years of age, of fairly strong constitution, and by occupation a seamstress, was admitted to the Glasgow Ear Hospital, on the 25th September last. She had suffered for six and a half years from a somewhat intermittent purulent discharge from the right ear, attended with occasional attacks of severe pain in the ear. On admission, the condition of her ears and hearing was as follows:—The right ear contained foetid purulent secretion, and on removing this, the tympanic membrane was seen to be perforated at the upper and back part, some granulation tissue protruding out of the perforation. In the left ear there was no purulent secretion, but a dry perforation existed, indicating a past suppurative disease of that ear also. In right ear a watch, heard in ordinary circumstances 40 inches from the ear, was audible only on contact. The bone conduction of sound was good, the watch being heard distinctly on temple; while the tuning fork, applied to the middle line of the head, was heard much more loudly on the right side; it was also heard longer when pressed on the mastoid process, than when conducted by the air. There was no redness over the mastoid region, while neither pressure nor tapping excited pain.

The course of the disease may be conveniently divided into three stages:—(1.) The period before the intermission; (2.) The period of the intermission; and (3.) The period of relapse, ending in death.

The first stage of the disease lasted for 28 days. About the 7th September, the illness began with headache, extending over the whole of the head but chiefly in the forehead, and sleeplessness, symptoms which, however, passed off in a few days. After remaining apparently well for two or three days, she became suddenly affected with constant and severe vomiting, described as bilious, and continuing for three days. The pain of the head also returned, and was described as "terribly bad," having now its seat chiefly over the right side of the head and the right temple. There was also remarkable drowsiness; the

constant tendency to fall asleep being only interrupted by paroxysms of pain in the head. There was likewise excessive yawning. The appetite was lost, and the bowels were obstinately constipated, being with great difficulty acted upon by medicines. On the 14th of September she went for the last time to the warehouse where she was employed, and was very ill the whole of that day. These symptoms, with exception of the vomiting, continued till the date of admission to the hospital. When admitted, her temperature was 98·4°. She was very drowsy, yawned frequently, and complained greatly of her head. The pulse was on the evening of the day of admission slightly irregular. Her tongue had a reddish-brown appearance. The abdomen was somewhat retracted.

During the next ten days, when she was under close observation in the hospital, her state was pretty much as follows:—

The *pain in the head* was always, if patient asked, complained of. When the side of the head was tapped there was evidently distinct tenderness over right temple, but none over mastoid process. The pain, however, disappeared completely on the 3rd October after a (fly) blister had been applied to right side of head.

*Drowsiness* was the prevailing feature. She was, however, at no time incapable of being aroused. Towards the end of the ten days the drowsiness gave way to cheerfulness and liveliness. At times there were slight delirium and restlessness.

*Paralytic Phenomena.*—Internal strabismus and slight drooping of the upper eyelids (ptosis) of right eye were noticed on 29th September. Two days afterwards some degree of internal strabismus of left eye was also observed. On this day Dr. Reid examined the eyes, and reported as follows:—"Paralysis of external rectus of left eye. Both optic nerves somewhat full, so far as can be made out under the circumstances, not well defined, and with traces of exudation on their surfaces, chiefly in the course of the vessels, which are slightly congested." The ptosis in right eye became, after a few days, much less marked, while the paralysis of external rectus of left eye remained very distinct. On 5th October the ptosis was quite gone.

*Pulse.*—The pulse varied from 52 to 60 for the first four days; after that it varied from 72 to 96. After blister it rose for a short time to 120. The elevation of the pulse corresponded with the improvement. Only on one observation was *irregularity* of the pulse noticed.

*Temperature* varied from  $97^{\circ}$  to  $98.6^{\circ}$ ; only on one occasion was it as high as  $99.2^{\circ}$  (after blister).

*Respiration* varied from 18 to 24. On one occasion, after the blister, the respirations numbered 36.

*Digestive System.*—The tongue presented during this term a dry, reddish-brown appearance, resembling that of enteric fever. The bowels were persistently constipated. On 30th September a sample of urine was furnished to Dr. W. F. Somerville, who made an exhaustive analysis of it. The chief changes found were diminished chlorides; increase of phosphates, especially of the earthy phosphates; and faint traces of sugar and albumen.

The ear was regularly and carefully cleansed with a solution of boracic acid, while the powdered boracic acid was insufflated after each cleansing. In addition the hair was clipped close to the scalp, and at first iced applications were used. On 2nd October Dr. Gairdner saw the patient, and on his suggestion a large fly blister was applied to the right side of head, which "rose" well, and seemed to be followed by good effects. The improvement, indeed, showed itself on the day following the use of the blister. Internal remedies were confined to the use of laxatives. Rhubarb and bicarbonate of soda, castor oil, and large enemata were all employed to overcome the obstinate constipation.

On 5th October, the second stage, or beginning of the intermission, began, when the following note was made:—"It is now four weeks since the first onset of the malady, and a remarkable improvement in the condition of the patient has manifested itself during the past two days. The ptosis is now quite gone. The head is free from pain. The patient is very cheerful. She is anxious to get up. The appetite has returned, and she insists that she "feels well." It is to be noted that this improvement corresponded with a slight rise in the temperature, and a considerable increase in the pulse. This marked improvement continued for two weeks, during the greater part of which she went about the institution, frequently saw her friends, and was quite vivacious and happy. The temperature during the first half of this time varied from  $98^{\circ}$  to  $99^{\circ}$ —pulse from 80 to 90. Unfortunately, during the second half of this time the condition of the temperature and pulse was not recorded. The paralysis of the external rectus of the left eye was almost the only indication that the recovery was imperfect.

During this interval, on the 13th October, Dr. Finlayson saw her and made the following notes:—"Still marked

paralysis of left external rectus (with internal strabismus in certain directions of the vision); no paralysis of the other muscles of either eye; no diplopia, as tested with candle in dark room. Perhaps a trace of ptosis on right side, but not very distinctly marked. A suggestion of a little flabbiness of left cheek brought out on laughing, but this not quite certain. Pupils somewhat dilated, but respond to light. Vision defective with small type; vision of right eye better of the two."

The third stage, or relapse of the symptoms, began on the 18th October, with headache (mainly in forehead), frequent vomiting, and drowsiness. On the following day the vomiting ceased, but the headache and drowsiness continued. On the 21st there was again vomiting and the hands and feet tended to be very cold. Blistering fluid was applied over the right side of the head, and a dose of calomel and scammony prescribed, which operated freely. These symptoms—namely, the headache, drowsiness, occasional vomiting, and occasional tendency to cold hands and feet continued pretty much for five days, that is, till the 23rd, when the left eyelids were observed to close imperfectly during sleep, while the right pupil was now seen to be decidedly more dilated than the left, both being sluggish. The drooping of right eyelid also returned. A momentary shudder was once seen to pass over body. The drowsiness also deepened into a condition from which she could scarcely be aroused, and she began to pass water in bed, which she continued to do till the end. On the day following there was slight twitching on right side of mouth, lasting for about half an hour. The left arm and leg were now paralysed, and there were occasional long sighing inspirations (Cheyne-Stokes). Quantities of sticky mucus collected about teeth and mouth. When liquid was put into mouth it either dribbled out again, or, after lying in mouth for a short time, it was swallowed, apparently unconsciously. Blistering fluid was at this stage applied to left side of head, and "rose" well. This seemed to be followed by some improvement, preceded by frequent yawning. When spoken to loudly, she now partially opened her eyes and answered in an incoherent way. She also slowly put out her tongue when requested, and asked for and swallowed some tea. There seemed also to be some slight return of movement of left arm and leg. Another dose of scammony and calomel was given to overcome the still persistently constipated state of the bowels. The medicine acted freely. On the 27th, Dr. Gairdner again visited the patient and regarding it as probably a case of intracranial

abscess, we agreed to ask the help of Dr. Macewen on the question, which was now raised, of the advisability of perforating the cranium in order to explore for pus in its interior. Dr. Macewen very kindly visited the patient at my desire, and was of opinion that there was a reasonable hope of reaching the seat of the disease (presumably a collection of pus) by trephining the skull above the ear, that is, opposite the lower part of the temporo-sphenoidal lobe. This he was prepared to carry out, but the suggestion was, for various reasons, not carried out.

The partial return of consciousness lasted for about three days, during which she drank milk and water freely. When asked persistently as to painful part, she raised her right hand to right temple. There were frequent flushings. The right arm and leg were in a state of almost constant motion, and the paralytic symptoms continued as before.

On the evening of the 29th, Dr. Finlayson again saw the patient, and reported as follows:—"Very sleepy and cannot be got to answer, but makes contortions if disturbed. While sleeping, the pupils are markedly unequal, the right being larger. When exposed to bright light, the *right* pupil is unduly wide, and no response to light is noticed. The left pupil is in a state of contraction exactly like that found in sleep, and on disturbing her it dilates; with such disturbance the right pupil also dilates a little more. Immediately on ceasing the efforts at rousing her, the left pupil begins to contract again."

Another sample of urine was furnished to Dr. Somerville for analysis. The most striking changes were increase in the phosphates, diminished chlorides, and faint traces of albumen.

On the 30th October patient could not be aroused. About 8.30 P.M. she suddenly became livid, and breathed at long intervals. I happened to visit her just at this time, she was blue, and breathing at the rate of about 4 respirations per minute. Pulse strong and rapid. The pulse, however, gradually became weaker, the respiration recurred at still longer intervals, and in about 15 minutes she was dead.

During the third or final stage of this illness, the temperature was somewhat higher than during the first stage, and varied from 99.2° to 100.2°—the latter only on one occasion—on the other occasions it never exceeded 99.6°. The pulse varied from 60 to 120. The slow pulse prevailed during the first few days, then with the partial recovery of consciousness it mounted to from 100 to 120; a day or two before death it sunk to from 72 to 84. Intermission of the pulse was only

noticed on two occasions. During this third stage also the tongue presented the same aspect as during the first stage, namely reddish-brown, and dry, still presenting as it did during the first stage the features of the tongue of enteric fever. There were also sordes on the teeth.

The *post-mortem* examination was made the day after death.

On opening the skull, a projection under the dura mater was seen a little to the right of the middle line of the vertex, corresponding with a thinning of the parietal bone, one inch from articulation with the frontal bone. On examination this was found to be only unduly prominent Pacchionian bodies, which were well marked in other situations. The pia mater all over was intensely injected and the convolutions were dry and somewhat glazed. On removing the brain and on cutting the membranes along the petrous bone, on the right side, a little thick, greenish pus was seen to exude, coming from just behind the petrous bone.

There was very little fluid accumulated at the base of the skull, and on examining the base of the brain there was no inflammatory exudation or matting together of the parts (though perhaps the fissures of Sylvius were a little unduly adherent). On looking at the base of the brain, it was apparent that there was a soft bulging or swelling on the temporo-sphenoidal lobe. This was felt to be distinctly fluctuant, and, on incision, a large quantity of thick, greenish pus came out of the cavity. This pus had a decidedly foetid odour, and measured about six fluid ounces. The abscess cavity, which was lined by a distinct and well marked lining wall, was found to present the following measurements—viz., three and a half inches longitudinally, two and a half inches in breadth, but the depth could not be ascertained. The lining membrane of the abscess had prevented the neighbouring brain tissue from being specially softened. On examining the ventricles, the structures seemed to be healthy and not softened, and the cavity only slightly, if at all, distended.

Hemispheres, as far as sliced, presented no signs of disease; but care was taken not to open into the abscess cavity, which was stuffed with cotton wool.

On examining the nerves at the base of the brain, with reference to any exposure to pressure, the third pair of cranial nerves, but especially that on the *right* side, from its great proximity to the abscess, must evidently have been pressed on.

Special examination of the sixth cranial nerve on the *left* side failed to find any special implication of it.

The dura mater was examined, but without finding anything; but it and the petrous bone were removed for special examination.

Both temporal bones, with the basilar process of occipital bone, were taken out in one piece. The dura mater over the right petrous bone was normal. The lateral sinus was permeable. The only trace of caries to be discovered from the intracranial aspect was a small aperture in the roof of the tympanum about two mm. in length. On cutting away the tympanic plate and exposing the outer surface of the tympanic membrane, an aperture, occupied partially by granulation tissue, was noticed in its upper and back part. On making a section through the whole length of the middle ear it was found that the mastoid cells were sclerosed and converted into dense, bony tissue; the only representative of the pneumatic cells of the mastoid process was a small cavity in the situation of the antrum. The tympanic cavity was diminished in size by evident hyperostosis of its walls. Caseous material occupied these cavities. The stapes was absent, the incus loose, but the malleus was still attached to the tympanic membrane.

The salient features of this case are:—

1. The long course of the disease—close upon eight weeks from the beginning of the symptoms.

2. After the symptoms had gone on for a month there was a period of apparent recovery, continuing for a fortnight, when the patient went about feeling cheerful and well, the only symptoms pointing to still existing intracranial mischief being paralysis of the external rectus on the *left* side, indicating mischief in the sixth nerve on the opposite side from the seat of the disease.

3. The state of the prevailing temperature and pulse was noteworthy. The temperature never exceeded  $99.2^{\circ}$  for the first six weeks. The pulse was generally slow in the greatest number of observations; the number of beats was no higher than from 52 to 70. During the final stage of the illness the pulse varied from 60 to 120.

4. The absence of shiverings was remarkable. The nearest to a rigor was a momentary shudder on one occasion. Probably the cold extremities which were noticed at the beginning of the relapse of the symptoms were analogous to shiverings.

5. No convulsive seizures occurred. Only a temporary



slight twitching of the muscles of the right side of the face was noticed.

6. The paralytic phenomena were important and interesting. The third nerve of the right side (dilatation of pupil and ptosis), the sixth of the left side (paralysis of external rectus), the seventh of the left (imperfectly closed eyelids, &c.), and the centre of the nervous supply to the upper and lower extremities of left side were all undoubtedly involved. With exception of the left external rectus, the other paralytic conditions were somewhat fluctuating, notably the ptosis.

7. It is worthy of note that the amendment in the condition followed close upon the blister to the head applied at Dr. Gairdner's suggestion.

8. Death took place by asphyxia, showing that the respiratory centre was at the last involved, probably by pressure.

9. Another noteworthy feature was the state of the tongue—reddish-brown and frequently dry, with mucus or sordes on teeth. The tongue was remarkably like that of enteric fever, and I notice in a case of cerebral abscess, recorded lately in the *British Medical Journal*, that an enteric rash was seen.

In the light of this case I would be disposed in future, if a patient presenting similar symptoms came under my care, to carry out the proposal of Dr. Macewen to perforate the skull, and thus give the patient a last chance.\*

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## CURRENT TOPICS.

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GLASGOW ROYAL INFIRMARY.—At the meeting of directors held on Monday, 1st August last, the vacancy in the staff of Visiting Physicians to the Hospital, caused by the retirement of Dr. Perry, whose term of office had expired, was filled up by the appointment of Dr. Samson Gemmell, Professor of Medicine, Anderson's College, Glasgow. All the other medical officers of the Infirmary were re-elected.

UNIVERSITY OF GLASGOW.—LIST OF DEGREES CONFERRED ON 28TH JULY, 1887:—

*Doctors of Medicine (M.D.)*.—I. *Commended for Thesis*.—Archibald Kerr Chalmers, M.B., C.M., Scotland (*Thesis*—"The

\* Since these notes were written a similar case has presented itself at the Ear Hospital, and has been operated upon by Dr. Macewen with a highly successful result. (See *Lancet*, 26th March, 1887.)

Temperature Curve of Specific Fevers"); Henry John Neilson, M.B., C.M., Scotland (*Thesis*—"On the Observation of the Pupil as a Guide in the Administration of Chloroform").

II.—*Ordinary Degree*.—Malcolm Black, M.B., C.M., Scotland (*Thesis*—"Puerperal Eclampsia, with records of 41 cases"); George Buchan, M.B., C.M., Scotland (*Thesis*—"Diabetes and its Treatment, a Clinical Study"); Anstruther Davidson, M.B., C.M., Scotland (*Thesis*—"An Analysis of Obstetric Cases, with observations"); Thomas Davidson, M.B., C.M., Scotland (*Thesis*—"Clinical Examination of Children, with indications for treatment"); William Little, M.B., C.M., England (*Thesis*—"Leaves in a Country Practitioner's Diary: 1, Maternal Impressions; 2, Cases of Pruritus Vulvae; 3, A Case of Calculus"); Islay Burns Muirhead, M.B., C.M., Scotland (*Thesis*—"Some Mental Approaches of Disease"); William Thomas Gardner Robinson, M.B., C.M., England (*Thesis*—"Indigestion as found among Fenmen").

*Bachelors of Medicine and Masters in Surgery (M.B. and C.M.)*.—I. *Honours*.—William MacLennan, Scotland.\*

II.—*High Commendation*.—Robert Galbraith Reid, Scotland; John Alexander Anderson, Scotland; Hugh Rhodes, England; James Anderson Goodfellow, Scotland.

III.—*Commendation*.—William Davidson, Scotland; Llewellyn M'Whannell, Isle of Man; Alexander Duncan, Scotland; Malcolm Alexander Mackintosh, Scotland; James Crawford Gibb Macnab, Scotland; John Macgregor Young, Scotland; Roderick A. Macleod, Scotland; Hugh Dickie, M.A., Scotland; James Adam, M.A., Scotland; James Marshall, Scotland; Thomas Joseph Redhead, England.

IV.—*Ordinary Degrees of M.B. and C.M.*—Frederick Ashurst, England; Thomas Cameron, Scotland; Robert Clarke, Ireland; Daniel Craig, Scotland; David Curle, Scotland; James Fraser Davidson, Scotland; George W. Davis, Scotland; John Dewar, Scotland; John Dickinson, M.A., Scotland; William Dinsmore, Ireland; Thomas Taylor Downie, Scotland; Alexander W. Dunlop, M.A., Scotland; John M'Intyre Eadie, Australia; John Edgar, M.A., B.Sc., Scotland; William Douglas Erskine, Scotland; Frederick Fedarb, England; Henry William Finlayson, Ireland; John Dyce Gordon, Scotland; Charles Christie Grant, Scotland; Robert Greenhill, Scotland; Archibald Gunn, Scotland; Thomas William Hay, England; Ernest W. Haydon, England; Robert Hogg, Scotland; Joseph Foord Hughes, England; George James Imrie, Scotland; John

\* Mr. MacLennan gains the Brunton Memorial Prize of £10, awarded to the most distinguished Medical Graduate of the year.

Archibald Jackson, England; Arthur Herbert Jago, England; Thomas Wilson Jenkins, M.A., Scotland; David Jones, Scotland; Hugh Wallace Kilpatrick, Scotland; Thomas Kirkland, Scotland; Thomas Laird, Scotland; John Love, Scotland; John Mackinnon, Scotland; William H. M'Kinstry, England; Donald Maclean, Scotland; Alexander Lewis M'Millan, Scotland; William M'Millan, Scotland; John Roderick Marshall, Scotland; George Miller, Scotland; James Tarini Coomar Mitter, India; John Kerr Morton, Scotland; Robert Barclay Ness, M.A., Scotland; William F. Ness, M.A., Scotland; Alexander Glen Park, Scotland; John Porter, Scotland; John Reid, Scotland; Andrew H. Richmond, Scotland; Robert Robertson, Scotland; William Robertson, Scotland; John Samuel Rosser, Wales; William Russell, Scotland; John Scobie, Scotland; Andrew Sprott, Scotland; Robert Stevenson, Scotland; John Mitchell Young Stewart, Scotland; Robert Stirling, Scotland; David Stone, Scotland; Edward Boyle Tant, Scotland; George Thomson, Scotland; James Alexander Ure, Scotland; William Walker, Scotland; William Wallace, M.A., Scotland; Dugald Campbell Watt, Scotland; Charles M. Wildridge, France; Edward Williams, Wales.

**IMPORTANT TRADE MARK CASE.**—At the instance of the Apollinaris Company, Limited, Mr. Justice Chitty, on 29th July, granted an interim injunction against the Agents of Kronthal Water, restraining them, their servants, and agents from using the word "Apollinis" upon any mineral water. Mr. Romer, Q.C., and Mr. J. Cutler appeared for the Apollinaris Company. Mr. Aston, Q.C., and Mr. Morshead appeared for the defendants. It appears from the evidence the Kronthal Water was originally sold in this country under the name Kronthal; but that recently the defendants adopted a label on which the water was described as coming from the Apollinis Spring. The defendants failed to justify their employment of the word "Apollinis." The court held that the word "Apollinis" was an infringement of the plaintiffs' registered trade mark "Apollinaris."

## Obituary.

## DR. ANDREW FERGUS.

BEFORE the present issue of our *Journal* reaches the hands of its readers, all or nearly all of them will have heard of the death of DR. ANDREW FERGUS. The pulmonary ailment which carried him off, on the 27th July, was sudden in origin and of short duration; but his intimate friends had, for several months previously, thought that they noticed indications of failing strength. Any forebodings, however, which such observations at times gave rise to were easily allayed in view of his invariable good spirits and constant energy—and so the shock to all was sudden and unexpected.

He was not so old a man as was generally supposed, having been born at Newcastle, where his father was Presbyterian clergyman, in 1822; and about thirteen or fourteen years afterwards he began his connection with the medical profession under a brother, who was then in charge of a parochial hospital in London. He used often to refer to those days, in which, as a very young boy in a "jacket without tails," he gave some form of assistance to his brother in his hospital, as well as to a period a few years later in point of time, when he was apprenticed to a busy and much respected practitioner of South Shields, because they left upon his mind evidently a strong impression of the value of the knowledge he then and thus acquired. Although dying at sixty-five years of age, it will be seen that he served in the profession, as man and boy, for quite half a century.

In boyhood he attended University College School, London, and his medical education was obtained at Glasgow University and King's College, London. The qualification which admitted him to the profession was the membership of the College of Surgeons of England, and was obtained in 1845. Two years later he began practice in Glasgow, and was appointed, during the same year, one of the district surgeons of the city. This appointment gave a bent to his mind which influenced much of his future work. It furnished to him an opportunity of investigating the epidemics of typhus and relapsing fevers then prevailing, as well as one of cholera, which broke out in the following year. The experience acquired then was put to practical use when cholera reappeared in 1854-55. Having been convinced that it was curable, in its very early stages,

by means of opium and absolute rest, he now successfully tested this opinion in his private practice. Further, he placed the workmen of two large public works (one of them in a bad cholera district) under the surveillance of their foremen, who had full instructions to carry out the treatment advocated at the first threatening of diarrhoea. Experience, resulting from this private and public practice, convinced him of the soundness of his views; and when, in 1866, Glasgow was again in danger of a cholera visitation, he reiterated his opinions for the benefit of his brethren by reviewing a work on cholera in one of the medical journals. This article was, by request, republished in pamphlet form, and ran rapidly into a third edition. In the same year he received from the University of Glasgow the honorary degree of Doctor of Medicine.

About this time also, finding that Glasgow, although abundantly supplied with pure and uncontaminated water, was still largely affected by diseases arising from excremental pollution, he turned his attention to the investigation of the matter, and came to the conclusion that the diseases in question were caused by impure air arising from the decomposition of sewage. Further investigation led him to what is now universally regarded as an important discovery in sanitary science—namely, the association of diseases of this sort in a family with the corroded state of the soil pipes in the house; the corrosion being the effect of the action of sewer-gas on the metal. He read papers on this subject in Glasgow; at the Social Science Congress in Newcastle in 1870; and, again, at Leeds in 1871. At first his views were by no means generally accepted; the tone of the discussions which followed his various communications on the subject indicated that he had taken an exaggerated view of the evil, and the opinion, especially of engineers, seemed to be that sewer-gas was almost entirely innocuous. The serious illness of the Prince of Wales from typhoid fever, presumably contracted in consequence of sleeping in an apartment in too direct communication with the sewers, greatly altered public opinion in the matter; and Dr. Fergus lived to see his convictions fully accepted and endorsed by scientific men.

From 1870 to 1874 he was a member of, and took an active interest in, the Health Committee of the Town Council. He retired from this position on being appointed President of the Faculty of Physicians and Surgeons (of which he had been a Fellow since 1851) in October of the latter year; but

he never ceased to take a warm interest in all matters connected with the public health of the City.

In the same year (1874) he acted as one of the local secretaries of the Social Science Congress during its session in Glasgow; and by request he opened a discussion on the disposal of sewage in the Health Section. On this occasion he enunciated, and gave a practical demonstration of what he used to regard as his second discovery on this subject—namely, the ready passage of sewer gases through water; a practical inference being the sanitary inefficiency of water traps. He took a leading part in all the preliminary arrangements for receiving the British Association in 1876, and acted as one of its local treasurers. At this meeting he again brought forward a communication on his favourite subject.

He was thrice elected to the Presidency of the Faculty of the Physicians and Surgeons by his brethren, an honour rarely bestowed, and very highly valued by him.

For many years he took an active part in the proceedings of the Glasgow Philosophical Society, of which he was successively Member of Council, Vice-President, and (on the expiry of Sir William Thomson's term of office in 1877) President. This position he held for the usual term of three years.

In October, 1877, by advice of the Privy Council, Her Majesty was pleased to appoint him her representative for Scotland in the General Medical Council of Education and Registration. He was reappointed on the expiry of his first term of office, and was an influential member of that body at the time of his death.

He was invited to give the address on Public Health at the annual meeting of the British Medical Association at Cork; and was lately appointed, by the unanimous vote of a meeting of his brethren in the West of Scotland, to be Chairman of the Executive Committee charged with making arrangements for the proposed meeting of that Association in Glasgow next year. This latter honour he declined, from a feeling that his strength was not such as it used to be.

He acted for many years as a Director of Anderson's College, the Royal Infirmary, the Western Infirmary, the Eye Infirmary, and many other charitable and philanthropic institutions.

The facts of his life, thus shortly stated, are sufficient to indicate that, although he could hardly be said to have yet entered on old age, he had already succeeded in accumulating, as few men ever do, "that which should accompany old age

—as honour, love, obedience, troops of friends." With the public of Glasgow he exercised a personal influence such as few medical men, however successful, ever possess; while the profession has lost in him one who was courteous and kind in his demeanour towards all, and especially helpful and friendly to those who were young and just beginning life. In this he set an example which we shall all do well "not to leave out of the lesson of our lives."

Much of his kind and happy mode of maintaining intercourse with others arose from his invariably cheerful and sanguine disposition. Even when, late in life and in the contemplation of early and well earned rest and retirement, he suddenly and unexpectedly found himself "out of suits with fortune," he appeared to forego none of his wonted cheerfulness. His most intimate friends saw no change in the spirits or the activity of the man. Those, moreover, who knew him best knew well that this was due to no affectation, still less to any callousness in his feelings. It was rather due to a power of rightly and heroically adjusting them.

A life like this, full of manifold activities, touching the lives of others at numberless points, respected and depended upon by many for advice and helpful services, is the sort of life which leaves behind it always the greatest sense of missing, of loss, and of consciousness of absence. We can hardly believe that he is gone, and with him so much of hearty service and willing co-operation. Yet this is the true gauge of the valuable influence his life exercised upon others. To his widow, family, and intimate friends such feelings, however painful at first, will make up in the end much of their surest solace. None need be ashamed of or desire to check the expression of them. *Quis desiderio sit pudor aut modus tam cari capitis?*

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## REVIEWS.

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*An Introduction to the Study of Embryology.* By ALFRED C. HADDON, M.A. (Cantab); M.R.I.A.; Professor of Biology in the Royal College of Science, Dublin. London: Charles Griffin & Co. 1887.

THIS is a work which shows very considerable ability, and is obviously written by an enthusiastic student of embryology. As a book of reference, it will be found full of hints, contain-

ing more or less distinct allusion to important matters of great variety, together with painstaking accounts of many views and allegations which, we cannot think, have at all a sufficient claim to be put forward as facts. In the preface, the author states that, although there are at the present time two students' manuals in the English language solely devoted to the study of embryology, it has appeared to him that a relatively small work, giving a general review of the subject, might prove of use to students. He proceeds to say that "a knowledge of the main facts of comparative anatomy and systematic zoology has been assumed for the reader, the book being specially designed for medical students." Now, if it is imagined from this and from the title, "Introduction," that in this work the student who has started in biology from the study from man and vertebrata generally, and wishes to add to his knowledge of adult forms and textures an acquaintance with what is thoroughly ascertained in embryology, by studying a shorter and simpler work than either of the two with which the name of the late F. M. Balfour, to whose memory this book is dedicated, was associated—if it is imagined that such an one will find in this handsome and clearly printed volume what he desiderates, a great mistake will be made. This is rather a supplementary volume, in which things well ascertained are hurried over, an acquaintance with the terminology being, even to some extent, apparently assumed, as, for example, in the description of the amnion and allantois; while, on the other hand, it is full of things which might, in an introduction, be missed out. Farther on, in the preface, we are told that "the facts of development have been largely supplemented by hypotheses;" and that is a description which is thoroughly true, in the largest sense. But when we are told that "the beginner is advised to pay attention only to the large type in the first reading, as purely theoretical subjects or matters of detail are printed in the smaller type," we hardly expect to find in the large print the facts and hypotheses inextricably mingled, particularly when we are informed that "an endeavour has been made so to present the latter that the student could not mistake them for the former. The following passage, beginning on the first page, is worthy of attention:—

"The second, or comparative aspect (of embryology), compares the development of animals, and taking those phases which are common to all or to many, attempts to reduce or reconstruct the evolution of the animal kingdom. This study is known as phylogeny. The chief result of all embryological inquiry has been to demonstrate



that the history of the individual recapitulates, in its main features, the evolution of the race, and thereby to give positive evidence in favour of the theory of evolution, in the general acceptance of the term."

We object, first, that comparative embryology ought to be as free from hypotheses as any other observational study. Its province is simply to compare, and when the comparison is not obvious, a provisional hypothesis of the relationship of different facts has to be made, which further observation will remove from the region of hypothesis either by confirming or refuting. What zoologists have got into the habit of calling phylogeny is a very good thing in its way, but it is not comparative embryology. It is necessarily highly theoretical, and its theories ought to be founded on a great deal more than embryology. Neither is the second statement which we have quoted strictly correct. The doctrine that the history of the individual recapitulates the evolution of the race originated with Serres a long time ago; and whatever truth there is in it he deserves credit for. But, in point of fact, we are not entitled to go further than to say that many things found in adults of lower form are represented in a measure in the embryo in higher animals, and that this might be expected on the supposition of common ancestry.

The first chapter is devoted to "Maturation and Fertilisation of the Ovum," which is done well; the second to "Segmentation and Gastrulation;" the third to "The Formation of the Mesoblast;" and the fourth to "General Formation of the Body and Development of the Embryonic Appendages." Then follow three chapters on organs derived from the epiblast, hypoblast, and mesoblast respectively, much the longest chapters; and lastly, comes an eighth chapter, devoted to general considerations. The arrangement might be improved. So many organs belong to more than one layer of the embryo that awkwardness results from using the layers in classifying organs. Among the things that might have been, with advantage, missed out, may be mentioned the assertion of Professor Huxley, supported by Miss Munn, that the enamel is mesoblastic. Those who remember the grounds of the assertion will recollect how groundless it is, while it obscures the meaning of one of the most curious developments of the elongated deep cells of the cuticle. The figure given from Wiedersheim leaves nothing to be desired in the way of proof of the epidermal origin of enamel. The account of the development of the eye is poor. We do not understand why Mr. Haddon only acknowledges, with seeming reluctance, the

development of the bacillary layers in continuity with the epithelial lining of the ventricles, and omits to mention that the hexagonal epithelium is the similar lining of the posterior half of the primary optic vesicle. But he has plenty room to dilate on the epiphysial eyes of lizards, a structure which consists entirely of the altered lining of the ventricles, and justly excites attention, but which is not yet sufficiently demonstrated to be an eye at all. The bibliography at the end of the book refers to a number of very valuable memoirs, and also to a variety of more or less trashy performances, some of them with no pretence to being original, and others not in the least embryological. We shall not be sorry to see this book reach a second edition, and when that edition comes, we hope it will be much improved.

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*Health Resorts at Home and Abroad.* By M. CHARTERIS, M.D. Second Edition. London: J. & A. Churchill. 1887.

THIS work contains a large amount of most interesting material in a very readable form. Dr. Charteris' style is always pleasant, and we have in addition an occasional passage of great descriptive power and eloquence such as we seldom meet with in a medical work. As an example of this we may cite the account of the pump-room at Carlsbad, in which a highly descriptive picture is presented to the reader in a few sentences:—

"In the centre of the room a big round cauldron is observed, from which, propelled by subterranean force, a stream of water is thrown up, four or five feet high, with a convulsive thrilling throb. This stream descends in scattered spray, and a cloud of steam hovers over the cauldron and rises to the roof of the building in which it is enclosed. The continuous restlessness of this subterranean pump, the hot sensation imparted by its vapours, the peculiar hissing and throbbing of the upheaved waters, fill the mind, even in the nineteenth century, with wonder and awe, and one can still fancy how centuries ago its origin was considered divine."

The book begins with a classification of the various kinds of mineral waters and of health resorts. While very brief these descriptions are admirably clear and sufficiently full for ordinary purposes. This portion of the work only occupies about eight pages, and we are immediately introduced to the various health resorts *seriatim*. Beginning with Bath, we have several pages devoted to the subject of massage, which

seems to be a speciality of modern Bath. The process of massage as practised here is described from the personal observation of the author, who was "massed" (to use a word which our author surely introduces inadvertently) by an operator whose character reminds us of the descriptions of the voluble barbers of old. "Like a society paper he enters into personal details of the characteristics and physique of eminent men he has massed, and tells how his work leads him into the palaces of the Czar and the homes of our own nobility." Most of our readers will sympathise with our author in his protest against the statement that "it requires lectures and two years' instruction to become a masseur or a masseuse." "An intelligent man or woman could easily understand how to mass expertly in say five weeks. All that seems to be required is to know the superficial anatomy of the body and to have firm yet soft hands, with pleasant and gossipy conversational powers."

The health resorts at home having been described, the principal ones on the Continent and in America are considered. There is a short chapter on sea voyages, and this is followed by an alphabetical classification of the health resorts, each being accompanied by a brief notice of the character of the place, and some useful hints as to accommodation and other practical matters. This list, although long, is scarcely complete, but sufficiently so for most purposes.

To add to the usefulness of the work we have several sections which will be of great service to the medical man in dealing with foreign methods of prescription. There is one section on the metric system, in which after the due explanations several examples of prescriptions made out in that system are given. Then we have an alphabetical history of the chief drugs with their doses in the Austro-German (metric) system and according to the British pharmacopœia. This is followed by a table of "Through Rates and Fares" from London to the principal health resorts on the Continent, and the cost of certain sea voyages. Lastly, we have a "Therapeutic Index," in which the various diseases are referred to the proper curative resorts. We must not omit to mention a map in which the Continental health resorts are indicated by red ink. Looking at this map it seems as if, in Germany at least, there are few places which are not within very easy reach of a health resort. In fact, the Germans have obviously made a distinct speciality of this department of therapeutics, as shown by the remarkably

complete and detailed "Bäder-Almanach" issued by a German publisher.

Altogether we may say that Dr. Charteris has succeeded within the compass of 200 pages in giving an extraordinary amount of information on a subject in which most practitioners are more or less interested, and he has done it without in any degree sacrificing clearness and readability to brevity.

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*A Text-Book of Medicine: for Students and Practitioners.*  
By DR. ADOLF STRÜMPELL, of Erlangen. Translated by  
HERMAN F. VICKERY, A.B., M.D., and P. C. KNAPP, A.M., M.D.;  
with Editorial Notes by F. C. SHATTUCK, A.M., M.D.  
London: H. K. Lewis. 1887.

At the present day there is no want of excellent English works on the science and art of medicine, which may be used by the student for educational purposes or by the practitioner for reference. In the very front rank of these must be placed the handsome volume now before us by Dr. Adolf Strümpell, now of Erlangen and formerly of Leipzig, for an excellent translation of which the English speaking profession is greatly indebted to Drs. Vickery and Knapp. In its mother tongue the work has in a comparatively short space reached a third edition, and we wish it a like success in ours. As is to be expected, the book is essentially German—German dogma and practice everywhere predominating. Especially is this true of the pathology of the volume, and the reader constantly recognises the influence of the teaching of Virchow, Cohnheim, and the other great masters of pathological science of the Fatherland. Here and there, however, the editor gently interpolates a note calling attention to the work of American or English physicians. There is no attempt in the volume to combine instruction in the elements of clinical medicine or physical diagnosis with that in purely systematic medicine, so that the book is chiefly intended for senior readers or for those who may be making use of practical text-books at the same time. The different articles are full and exhaustive, each description including a detailed account of the etiology, pathological anatomy, symptoms, and treatment of the disease. The section on Diseases of the Nervous System is much the largest in the book, occupying nearly 300 pages of the whole, and it is this part of the work which has reached the third edition in the original. All the most recent facts and theories connected with neuro-pathology are embodied, and those who become

possessors of this volume will have at hand a reliable book of reference for this important group of diseases. Here and there we observe a certain inequality as regards the amount of space devoted to the description of different diseases. Thus, although 26 pages are given to the consideration of typhoid fever, typhus is disposed of in three, a circumstance which is probably explained by the much greater prevalence in Germany of enteric as compared with typhus. Again, we notice that such subjects as Interstitial Myocarditis, Primary Idiopathic Hypertrophy of the Heart, Arterio-Sclerosis, &c., are more exhaustively discussed than is usually the case in English treatises, and the frequent association of the former condition with a morbid state of the coronary circulation is fully insisted upon. The volume contains 111 illustrations, which, as a rule, are exceedingly well executed.

We have thus attempted to give our readers some idea of the character and scope of Dr. Strümpell's book, and have only to add that by its translation we have gained an addition to our national medical literature of lasting value.

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*Post-Mortem Handbook; or, How to Conduct Post-Mortem Examinations for Clinical and for Medico-Legal Purposes.*  
By THOMAS HARRIS, M.D. (Lond.), M.R.C.P. London: Smith, Elder & Co., 15 Waterloo Place. 1887.

WE have pleasure in recommending this little volume to our readers as one which is likely to prove most serviceable both to students and practitioners. The medico-legal hints which are everywhere insisted upon constitute a feature in the volume, and will prove of great value to practitioners who are called upon to perform *post-mortem* examinations, the results of which are to be received as evidence in courts of law. "The method adopted is essentially a modification of that of Virchow," which is carried out all over Germany, and which is remarkable on account of the completeness with which it permits of every part of the body being thoroughly scrutinised. The German method is that which we ourselves always employ, although here and there we differ from the recommendations of the author in points of detail. Thus, it is our practice always to use the cartilage knife in opening the sterno-clavicular joint, and we certainly think that the beginner will spoil a good many sharp scalpels if he adopts the advice of Dr. Harris. In describing the difficulties of dealing with adhesions of the lung, the author does not mention the ease with which

they may often be overcome by cutting through the costal pleura and stripping it off when removing the organ; and we cannot agree with the advice that it is better to slit open the bronchi and pulmonary vessels before incising the lung. The plan of washing the mucous membrane of the intestine by passing the bowel between the handle and the edge of a pail is certainly ingenious, but it is a procedure which can only be defended on the plea that the operator is unwilling to soil his hands. By such a method we think it possible to injure many pathological appearances met with on the intestinal mucous membrane which would not be in the least harmed by careful manipulation with the fingers; and by keeping an abundant stream of water flowing over the fingers as well as the mucous membrane, they are much less soiled than one would easily believe. We have thus referred to a few points in regard to which we differ from the author; but, taken all in all, the book is well done, and we heartily recommend it.

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*Transactions of the New York State Medical Association for the Year 1886.* Volume III. Edited for the Association by A. L. CARROLL, M.D. Concord (N.H.): Republican Press Association. 1887.

*Transactions of the Association of American Physicians.* First Session. J. Delafield, M.D., President; James Tyson, M.D., Secretary; J. T. Whittaker, M.D., Recorder. Philadelphia. 1886.

THE enterprise of our professional brethren in America is well illustrated by the two handsome volumes now before us, both of which contain many contributions of great scientific and clinical value. The former volume contains records of discussions on the three following subjects, viz.:—(1) On Shot Wounds of the Intestines; (2) on Pulmonary Tuberculosis; (3) on Eclampsia. As we are in the habit of holding discussions, more or less successful, in Glasgow, it may be interesting to some of our readers if we shortly advert to the American method of conducting the debate. The proceedings are briefly opened by a gentleman appointed for the purpose, who indicates in a general way the lines upon which the discussion is to proceed. A series of practical questions on the subject under consideration are then propounded, the task of answering which in detail is delegated to certain gentlemen, who are then followed by any other member who may feel himself called upon to take part in the discussion. Such a method seems to

us to possess many advantages, chief amongst which is the circumstance that it concentrates the attention of those present on the great practical points, and prevents that wandering away into side issues and into the ventilation of pet theories which has detracted so materially from the value of some of our recent discussions. In the same volume are contained memoirs (accompanied by excellent photographs) of the late Drs. Austin Flint, Frank H. Hamilton, and John P. Gray.

In the second volume, among many valuable contributions, we may simply mention the following:—"The Tendon Jerk and Muscle Jerk in Disease, and especially in Posterior Sclerosis," by Weir Mitchell and Morris J. Lewis; "An Experimental Study of Glomerulo-Nephritis," by W. H. Welch; "The Bicuspid Condition of the Aortic Valves," by W. Osler; and "The Bacillus of Typhoid Fever," by G. M. Sternberg.

*What to do in Cases of Poisoning.* By WILLIAM MURRELL, M.D.  
Fifth edition. London: H. K. Lewis. 1887.

DR. MURRELL'S excellent little book has increased slightly in size—a fact which the author deplores, but which he nevertheless justifies on the ground that "There are too many poisons now-a-days; if people who contemplate suicide would only adopt a uniform method, it would facilitate matters greatly." The work remains, however, small enough to be conveniently carried in the pocket or instrument bag, while in comprehensiveness, in lucidity and plainness of detail with regard to treatment, and in the total absence of "padding" or other unnecessary material, it easily maintains its position as the most serviceable handbook on the subject of which it treats.

Scattered through the book will be found much practical and valuable advice, particularly in all that relates to the legal aspects of poisoning cases. Such statements as the following should prove consolatory to young and nervous witnesses:—"Do not imagine that the object of cross-examination is to elicit the truth. Do not be afraid of cross-examination. If you have read up your subject you should be more than a match for any barrister. His knowledge is of necessity superficial, and he is hampered by the fear of displaying his ignorance." And yet witnesses *will* be nervous when in the witness box!

Dr. Murrell has a pleasant humour which, it is satisfactory to note, is not incompatible with authorship even in this

gloomy walk of literature. Evidence of this will be found more especially in his earlier chapters and in the preface, in which latter he tells his tale of the would-be suicide who, being of a retiring disposition, changed his mind on reading the directions for treatment!

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*On the Diagnosis of Diseases of the Brain, Spinal Cord, and Nerves.* By C. W. SUCKLING, M.D. (Lond.), M.R.C.P. With illustrations. London: H. K. Lewis. 1887.

THIS little book is worthy of being well known to all members of the medical profession. In these days, when our knowledge of the signs and symptoms of nervous disease is advancing with very rapid strides, the busy practitioner must often feel that, engaged as he is in general medical and surgical work, he is being somewhat left behind. It is for the help of such that the present volume is designed, and we confess that it caused us some surprise to find how well the author had succeeded in compressing so much valuable information into so small space. In fact, in some places perspicuity is just on the point of being sacrificed to conciseness; but having said this, we have said all we wish to say in the way of criticism. The volume is based on a post-graduate course delivered by the author in Queen's College, Birmingham, and we think he has done well in acceding to the request of his pupils by publishing his lectures in the present form, and thus enlarging the circle of those who are likely to benefit by his labours. The volume will also be of great service to the hospital physician, who, in minutely investigating an obscure nervous case, often feels the want of a handy volume to which he can speedily refer at the bedside. No one will regret adding the book to his library.

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*A Practical Treatise on the Diseases of the Rectum.* By ALFRED COOPER, F.R.C.S., Surgeon to St. Mark's Hospital for Fistula and other Diseases of the Rectum, &c. London: H. K. Lewis. 1887.

THIS small book purports to be a "concise and thoroughly practical treatise on the diseases of the rectum," but its conciseness is such that we venture to think it contains little more than will be found in any ordinary good text book of surgery. The book contains little that can be called novel, and therefore affords little to criticise. The author, in contrasting the various operations for the removal of internal



hæmorrhoids, speaks highly of the operation of ligature; and in comparing it with the use of the clamp and cautery, observes that "secondary hæmorrhage is more likely to occur than after the use of the ligature; and, as compared with the latter, the results of the operation are not satisfactory." We are not surprised at this conclusion if the author follows out his own practical injunctions of using the cautery iron "heated to a white heat." The cautery used at a *dull red heat* has been found by those who invariably adopt this method of treating piles to be followed by the best results.

The chapter on the Use of Electricity in Rectal Surgery, written by Dr. W. G. Stevenson, contains much useful information.

The book may serve a useful purpose in bringing more pointedly the subject before practitioners; but from a specialist of "twenty years' experience" we confess to a feeling of slight disappointment that something better was not forthcoming, and we can only express the hope that Mr. Cooper may see his way in the future to produce something more worthy of his position and his experience.

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*The Mineral Waters of Vichy.* By DR. C. E. CORMACK.  
London: J. & A. Churchill. 1887.

THE thermal springs of Vichy have been long known to the profession as a very valuable means of treatment in diseases of the intestines, liver, and urinary organs. In his book, consisting of 365 pages, Dr. Cormack enters fully, and in a readable manner, into the methods of administering the waters internally and externally, and also gives details of various promenades in the environs. He states "that while almost every nationality is annually represented in a larger proportion, there is one exception to the general rule: the number of English tends to decrease, and is considerably less than it was ten years ago."

It is to be hoped that this work will fulfil the purpose of its author—to induce more of our countrymen to go to Vichy, and assist them to pass their time pleasantly while there.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY.

SESSION 1886-87.

MEETING X.—2ND MARCH, 1887.

DR. A. PATTERSON, *Vice-President Surgical Section, and subsequently* DR. HUGH THOMSON, *in the Chair.*

## OBSERVATIONS ON THIRTY CASES OF ABDOMINAL SECTION FOR OVARIAN AND OTHER CYSTIC TUMOURS.

BY DR. H. C. CAMERON. (See page 161.)

*Mr. Maylard* said that he might so far supplement the statement of Dr. Cameron in regard to the case of pancreatic cyst by mentioning that, in order to examine the case carefully at the *post-mortem*, he removed the pancreas, duodenum, aorta, and spleen. He found a ligature around the splenic artery; and he also found that the cyst was in direct connection with the pancreas. In passing his hand into the cyst, he found that the splenic artery was in direct contact with the cyst wall. It was impossible that the cyst could have been removed without at the same time removing half of the pancreas and spleen; and before this could have been done death would have supervened. With regard to ligature of the splenic artery, *Senn* mentioned a case of *Billroth's* which, in some of its features, resembled that of Dr. Cameron. In that case also the operator was obliged to desist: and the remark was made that it would have been impossible to diagnose the state of matters at the time of the operation.

He had seen other two cases, and in none of them had a diagnosis been made; in one of the cases the patient died of diabetes. With regard to treatment, that recommended in the admirable paper of *Senn* was not to attempt to remove the cysts, but to open by median incision and stitch to the wall.

*Dr. Knox* said that he had operated on six cases of ovarian tumour, in all of which there was recovery. The cases had been on the whole fairly simple cases, most of them occupying less than an hour, and the two last only twenty minutes, the greater part of which was taken up in stitching the wound in the abdomen. He never used the ligature, preferring the clamp and cautery. The cautery seemed to him to be an

almost perfect instrument, severing the pedicle so thoroughly and closing the vessels at a considerable distance from the point of division, and on removal of the clamp, there remained what nearly appeared to be a linear cicatrix. He still used the spray, only substituting eucalyptus for carbolic acid. In only one case had he used drainage, in that case about a couple of gallons of peritoneal effusion being removed before touching the tumour; and after the operation about 13 oz. drained away within a couple of days. In all the other cases in which there had been simple adhesions, Keith's plan of first clamping the adhesion and ligaturing outside the clamp was perfectly satisfactory. There was no hæmorrhage and no effusion into the cavity. For ligatures he had used sometimes catgut, but latterly silk boiled in a solution of carbolic acid glycerine. In one case he had operated soon after the patient recovered from an attack of scarlet fever, which was contracted immediately before or during the voyage from Ireland to come to the hospital. In one of his cases he was informed that though the operation was performed 18 months ago, the patient still suffered from pain in the back, and he was at a loss to know the cause or the treatment. In one case there was a good deal of bronchial affection, without pleurisy, which he had attributed to the ether used as an anæsthetic. He had no case followed by inflammatory lesions, or lesions connected with the removal of pressure.

*Dr. W. L. Reid* said that in regard to the occurrence of parotitis, he had met with it in a case of fibroid polypus of the uterus, in which the tumour was giving rise to a foetid discharge. On the tenth day after the operation parotitis took place, and she died from starvation in about a fortnight. As regarded operations connected with the opening of the abdominal cavity, he laid great stress on paying attention to feeding the patient. The injudicious giving of food not seldom led to disastrous results. His own experience of ovariectomy extended to seven cases, all of them recovering. Some of them were very complicated cases, and none of them could be said to be simple. But in such cases as some of those which he had described, in which the tumour was largely adherent, there would always be a considerable mortality.

*Dr. Gairdner* mentioned a case of a girl in the North of England, in which it had happened to him to have to advise the first step towards what turned out to be an ovariectomy in perhaps the youngest patient on which any one present had operated for such a cause. The girl, aged 10 years, had been rapidly emaciating, with an enormously distended abdomen,

like that of pregnancy. To the hand it felt very solid, and had grown to this extent within a few weeks, which it appeared hard to believe. It was impossible to arrive at a conclusion without a pelvic examination, a thing unpleasant to propose in so young a patient, and he said at once that he would proceed no further. But he said that it was either a tumour attached to the ovary, which could be cut out, or a malignant tumour of the uterus, and therefore to be let alone. The girl was taken to Dr. Keith, who made out a solid tumour of the ovary, which he dissected out with great difficulty. The girl was now in good health.

*Dr. Robert Perry* stated that he sent a case to Dr. Keith to be operated on, which was done successfully, and within two days after the operation the lady was dead. Dr. Keith attributed this mishap to the spray, the use of which he then discontinued. In another case Dr. Keith successfully operated on a case of his, and death took place within two or three days after her return, from an attack of peritonitis. Dr. Keith said that in that case the presumption was that she had overfed herself, he having experienced great difficulty in keeping her on restricted diet while under his care.

*Dr. Knox* supplemented his previous remarks by giving the details of a case in which the patient was removed by her husband after the operation, on the ground that she was being starved, and who was almost killed by injudicious feeding on the day of her removal.

*Dr. J. Anderson Robertson* put a question as to the length of the incision in Dr. Cameron's cases.

*Dr. Morton* said that they were especially indebted to Dr. Cameron for the record of his unsuccessful cases.

*Dr. Cameron* said that he had omitted to mention one form of complication in the operation of ovariectomy—viz., ventral hernia. In two of his cases this had occurred. In one the cyst was of the semi-solid multilocular kind. No fluid flowed from the cannula, and he had to make a free incision and drag up the tumour. The walls were so rigid, and sprang back so easily, that he could not deal with the adhesions with the first incision, and ultimately he split the abdomen from the pubis to the umbilicus. In getting over the chance of a ventral hernia there was a great advantage, in cases of simple adhesion, in a small incision; and this was equally advantageous in relieving the intestines. A long incision gave great advantage in dealing with multiple adhesions, but there was the chance of weakening the abdominal wall. Dr. Knox mentioned that he did not use drainage, and his six cases all recovered. His

own first six cases also recovered, but the seventh would have been lost without drainage. Yesterday he operated on an extremely severe case of cyst in the broad ligament, which was enucleated with great difficulty, involving a good deal of hæmorrhage. He put in a drainage tube, and many ounces of bloody fluid saturated the dressing. He thought this case would be successful. The question of drainage was to be determined in a given case by the septic or aseptic nature of the fluid. In regard to tapping, he had operated in only one case in which tapping had been frequently performed, the patient having objected to a radical operation. She had been tapped nine or ten times, when she got wearied with this, and then he removed from her the largest cyst he had ever taken out. The whole of the front part of it was intimately adherent to the peritoneum. Except for diagnostic purposes tapping was most undesirable. Senn's advice to stitch the pancreatic cyst to the abdominal wall was one which he could not follow, the operation having proceeded too far. The prolonged operation had exhausted the patient. How could a diagnosis be made in these cases? The cyst had no pelvic connection; it was anchored above, and could not be pulled downwards. Cysts in the region of the spleen, the kidney, and the pancreas, must always be liable to difficulty of diagnosis. As regards the question of the place for operating, his three first operations were performed in the hospital theatre. This was a course not to be commended for many reasons, the necessity of removal by hoist, the tainted atmosphere, &c., being disadvantages obvious enough. He believed that the place for operation in ovariectomy was the room in which the patient was to be put to bed, the preparation of which for the operation Dr. Cameron described.

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#### MEETING XI.—18TH MARCH, 1887.

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DR. MACLEOD, *President, in the Chair.*

I.—SOME POINTS IN RELATION TO THE DIAGNOSTIC SIGNIFICANCE AND THERAPEUTIC INDICATIONS OF LARYNGEAL SYMPTOMS, RESULTING FROM PRESSURE OF ANEURISMS UPON THE VAGUS AND RECURRENT LARYNGEAL NERVES.

BY DR. DAVID NEWMAN.

The main purposes of the paper were to show—1st. That aneurism of the aorta and of the innominate artery may exist,

and give rise to laryngeal symptoms only; but that, in most instances, on critical physical examination, certain collateral signs may be made out sufficient to warrant one in forming a positive diagnosis, or to give rise to a very strong suspicion of an intra-thoracic tumour. 2nd. That in the early stage pressure may cause paroxysms of most urgent dyspnœa, accompanied by laryngeal stridor and paroxysmal cough. 3rd. That at a later stage paralysis usually, but not always, limited to one side occurs, characterised by phonative waste of breath and imperfect cough, but without dyspnœa, except when reflex spasm is indicated on the opposite side, or when pressure-stenosis is caused by the aneurism. 4th. That in certain cases tracheotomy should be performed not only to prevent impending death from asphyxia, but also as a remedial measure.

In the course of the paper the four cases, of which the following are summaries, were described in detail:—

CASE I.—Aneurism of innominate artery and descending aorta: paroxysmal dyspnœa from unilateral spasm of right vocal cord, followed three months after by bilateral spasm of adductors, and at a more remote date, by bilateral paralysis of adductors. Dysphagia and laryngeal symptoms only early evidences of aneurism: appropriate treatment recommended but not carried out: increase in size of aneurism: death from direct pressure of aneurism upon trachea.

CASE II.—History and evidence of syphilis: when first seen, the only symptom was aphonia, from pressure on the left recurrent laryngeal nerve; and only other physical sign, beyond laryngeal indications, supporting diagnosis of aneurism, was a difference in strength and rhythm of radial pulses: within short interval development of paroxysmal tracheal dyspnœa, partial suppression of respiratory murmur on left side, and detection of dull areas in course of ascending and descending aorta: hypertrophy of left ventricle: during six months' residence in ward great amelioration of symptoms and diminution in size of aneurism.

CASE III.—Aneurism of descending portion of arch of the aorta: dyspnœa and dry cough, without alteration in voice: physical examination in early stage revealed only hypertrophy of left ventricle, without bruit: treatment neglected: increase in severity of symptoms: physical evidence of aneurism: continued dyspnœa, with paroxysmal exacerbations.

CASE IV.—Paralysis of the left vocal cord, with aphonia, and, on exertion, dyspnœa: suspicion of aneurism at bifurcation of the trachea: no increase of symptoms during six

months: improvement in voice by compensatory action of the right cord: latterly slight obstruction to deglutition.

These cases were selected for the purpose of showing that an aneurism of the aorta, or of the innominate artery, may exist, and give rise to laryngeal symptoms, for which the patient seeks advice. With the aid of the laryngoscope, the apparent laryngeal disease may lead to the discovery of an aneurism, which might not otherwise have been suspected; but the suspicion once having been raised, a critical physical examination usually reveals one or more circumstances which demonstrate the presence of an aneurism. The most obscure case is the one last described (No. 4). The age of the patient is in favour both of malignant tumour and of aneurism, and there are no symptoms to be observed which might not be present in both; but the situation of the disease, the slow development of the symptoms, and, with rest and iodide of potassium, the practically unaltered condition during the last six months, favour the opinion that the paralysis is due to the pressure of an aneurism.

But while the true nature of this case cannot be definitely decided, it serves to show the difficulties that may arise in certain circumstances. During the early observation of the other cases, in one of them, dysphagia and the laryngeal symptoms were the only evidences of aneurism, and in the other two, although it cannot be said that there was an entire absence of any of the physical signs or symptoms of aneurism, yet it may be asserted that the symptoms and signs, other than the laryngeal, if taken alone, would not have raised the suspicion of disease of the great vessels. But while, during the early state, this was so, it must be admitted that the phenomena which developed later on fully justified the diagnosis of aneurism.

After showing the bearing of physiological experiment upon the minute diagnosis of laryngeal neurotic lesions, Dr. Newman pointed out the method of diagnosing whether the obstruction to respiration was the result of interference with the function of the larynx, or was due to a diminution in the lumen of the larger air tubes. He also showed how to determine when the dyspnoea was due to paralysis of the abductors, and when to spasm of the adductors, and endeavoured to show that the laryngoscopic image or laryngeal symptoms may reveal the presence of an intra-thoracic aneurism before it is likely to be otherwise suspected, and that even after a critical physical examination of the thorax, no signs may be found, in the early stage, to confirm the diagnosis; but that, as a rule, collateral

signs, such as swelling, dull area on percussion, pulsation, thrill, *bruit*, variation in the pulses, inequality in the pupils, or difficulty in deglutition, either warrant one in forming a positive diagnosis, or strengthen the suspicion of aneurism.

In conclusion, Dr. Newman said, "as regards treatment, it will be acceded by all that early diagnosis and successful treatment go hand in hand. Therefore any, even the smallest, indication which will lead to the suspicion of an aneurism, and thereby cause a critical examination to be made of the facts of the case, must be a great assistance to the practical physician. Cases III and IV exemplify this.

"Spasm, or paralysis involving only one side of the larynx, very rarely causes serious difficulty in breathing in adults, but by direct pressure the original disease may diminish the lumen of the larger air passages, and give rise to tracheal dyspnoea. It is bilateral paralysis of the abductors, or spasm of the adductors, that causes serious impairment to respiration, for which surgical interference may be required.

"There is only one point in the treatment to which I shall allude. It is with reference to the question of tracheotomy in cases of aortic or innominate aneurism. It is impossible to deny that the paroxysms of dyspnoea not only act very deleteriously upon the aneurism by causing increased strain upon the walls of the sac, but are also an immediate danger to life. Therefore, placing a tube in the trachea will perform a double service—it will permit of rest, and relieve the patient of the danger of death from asphyxia, or rupture of the aneurism during a paroxysm. It may be said that such a proceeding can only give temporary relief; but when the dyspnoea is a consequence of laryngeal obstruction, and not a result of direct pressure of the aneurism upon the trachea or the lungs, under these circumstances it would be very difficult to draw a limit to the advantages which might accrue from enabling the patient to breathe freely.

"If, on examination, the lungs are found to admit air abundantly, and to be free from physical signs of disease, should the laryngeal obstruction become threatening, the sooner tracheotomy is performed the better. The question may be asked—What is the benefit of performing tracheotomy on a patient suffering from thoracic aneurism, a disease in which the tenure of life is very precarious? In reply, I may say that, as a clinical observer, and as pathologist to the Glasgow Royal Infirmary, experience has taught me that in a large number of cases of aneurism, death from hæmoptysis is preceded by threatenings of laryngeal suffocation; and in



many cases I have been convinced that the rupture of the sac has been directly caused by the spasmodic attacks of dyspnoea. It is *not only* when the patient is suffering from suffocating agony, and all the circumstances indicate that no time is to be lost, that tracheotomy should be performed. The operation, which, in itself, is not attended with danger, should be resorted to whenever it is evident that the patient's life may be placed in jeopardy by laryngeal dyspnoea, or when the laryngeal impediment to respiration is clearly causing enlargement of the sac, by undue strain upon the circulation.

"The tracheotomy tube should have an opening on its convex aspect, and be provided with a valve so as to enable the patient to breathe by the mouth, and speak, by occluding the tube with his finger. Such a tube may be worn without danger, or even much inconvenience. Intubation of the larynx is not applicable in such cases, but should be reserved for cases of laryngeal stenosis arising in the course of acute disease, and the removal of the paralysed cord, as proposed by Dr. G. B. Hope in cases of paralysis of the abductor muscles, is only mentioned, not recommended."

Dr. J. Wallace Anderson said that, in regard to Dr. Newman's first case, the throat appearances were not of very great importance, as there were otherwise adequate materials on which to found a diagnosis of aneurism of the innominate artery. The second case, however, was one in which Dr. Newman's observations were of great value. It was one of these cases to which Dr. Gairdner had adverted in his *Clinical Medicine*, regarding which one was continually in doubt as to its nature. When he examined that case first, some weeks ago, there was, he believed, a very slight dulness high up on the left of the manubrium, especially if the patient were disturbed, but this had now disappeared. Some of those who examined the case said that there was also dulness on the opposite side. It was a case in which one's judgment was apt to be carried in any direction by the opinion of the specialist. He would say that there was probably an aneurism of the outer part of the left half of the transverse portion of the arch, and perhaps of the descending portion. The left radial pulse followed the right very much as the one sound of the heart followed the other. There was a slight pulsation over the top and behind the sternum. The disappearance of the dulness would seem to show that there had been some contraction and consolidation of the contents. On going the other day to the bath-room, the patient was seized with such an attack of dyspnoea that he had to go back

to bed. A circumstance like this, pointing to something of the nature of bronchial asthma, was apt to throw one off his guard as to diagnosis.

Dr. Gairdner said that he was well entitled to bear testimony to the great value of the observations in the paper, having had painful experience of mistakes in diagnosis in pre-laryngoscopic times, by laryngeal disease being taken for aneurism, and aneurism, when so situated as to be placed beyond the pale of physical diagnosis, being mistaken for laryngeal disease. The first case given in his *Clinical Medicine* entirely took him in. After the first case he looked upon it as almost an aphorism that any case in which the fact of the existence of disease in the epiglottis or glottis was not recognisable by the finger might turn out to be a case of aneurism. He remembered very distinctly, when his mind was in that condition, Dr. Horace Green—who had a great reputation as a specialist in laryngeal affections, especially in connection with what was then called *swabbing* the larynx—was on a visit to this country, he (Dr. Gairdner) had a case rendered doubtful by the absence of any definite symptoms, which he asked that gentleman to examine. Dr. Green pronounced it to be a case of laryngeal disease, though he himself could not detect any such disease. The patient died, and it was found to be an aneurism. At one time he was much disposed to resort to tracheotomy in these cases, and on this subject several discussions took place in the Medico-Chirurgical Society of Edinburgh. Theoretically he was still of the same opinion, and if he saw a patient being throttled and strangled to death, he would perhaps recommend operation. At the same time his experience did not very much support this procedure as one of much utility, and if he were absolutely certain that the disease was an aneurism, he would very unwillingly resort to tracheotomy. Dr. Newman might be interested in hearing that, when Czermack came to Glasgow to show them the laryngoscope, he had a case which he was anxiously watching with Mr. Lister to see whether tracheotomy should be performed. The left vocal cord in that case was in a state of paralysis. He had prognosticated the case to be laryngeal, and this was verified by the laryngoscope. It was a point of great difficulty to say what circumstances determined paralysis in one set of cases and spasm in another set. Spasm was doubtless the dangerous condition. He always strongly insisted on imperfect cough as a symptom of grave significance. Sometimes across a ward he would hear a cough wanting in proper explosive quality, and this, standing even as an isolated

fact, would sometimes suggest or indicate to the practitioner the diagnosis of aneurism.

*The President* said that he had operated by tracheotomy in four cases, but in none of them with the least service. Two of the cases were, however, already almost in *articulo mortis*. He certainly would not care to be asked to perform the operation in such cases, as he would have no confidence that it would be of much use.

*Dr. J. Walker Downie* said that Dr. Newman's first case was interesting in the respect that the paralysis was bilateral, while in most cases it was unilateral. In the fourth case there was no dull area, and no distinct paralysis on the left side. He had a somewhat similar case, in which the patient complained of neuralgia of the left side of the neck. There was distinct paralysis on the right side. There were no symptoms of aneurism, and he put down the symptoms present to nerve exhaustion. Under a course of repeated blisters and nerve tonics the paralysis disappeared. The number of cases in which tracheotomy was permissible was limited. In the later stages it was a hopeless procedure. But in all cases in which there was the suspicion or possibility of pressure, it was right that the larynx should be opened.

*Dr. Middleton* said that in regard to Dr. Newman's second case he examined it and satisfied himself of the existence of dulness, with some interference with the respiratory murmur over a very limited area. He would say from the situation high up this would be due to an aneurism situated in the arch of the aorta.

*Dr. M'Intyre* said that having seen a considerable number of cases of tumours in this region, the difficulty of diagnosis in several of them had forcibly struck him, especially in the earlier stages of the affection. As regards diagnosis, the whole of the cases might be divided into two classes—those in which the laryngeal symptoms stood alone, or at all events, were not greatly confirmed by other symptoms, and those in which a diagnosis of aneurism could be made independently of the laryngeal condition. Since last December he had seen three cases of suspected aneurism. One of these had been under observation for seven months, and up to that time no sure conclusion could be drawn. The patient was a clergyman, and the affection for which he was first seen was a sore throat. He (Dr. M'Intyre) was requested to report on the laryngeal condition. He found that the right cord was in a state of paralysis, and that the left cord had crossed to the middle line. For two years he had been following his profes-

sion, under circumstances involving enormous vocal stress. As to treatment, one mode had not been adverted to in the paper or in the discussion. The passage of tubes at the time of the spasms had been of great service. This treatment had been extensively carried out in America, a record of 250 cases having recently been published. He had himself on several occasions passed these tubes, and with good results. He was not to be put down as depreciating the value of laryngoscopic examination. It might be useful in the early recognition of symptoms suggestive of aneurism. But standing by itself, laryngoscopic examination did not give adequate data for a diagnosis of aneurism.

*Dr. Newman* said that, in reference to *Dr. Gairdner's* remarks, the position he took up with regard to tracheotomy was, that it was of no use in the later stages, but only in the earlier period before the aneurism had increased in bulk. His position in regard to the importance of a laryngeal examination was that when the existence of an aneurism came to be suspected, then such an examination might be of essential service in confirming or negating such a suspicion.

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## MEETING XII.—1ST APRIL, 1887.

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DR. ROBERT PERRY *in the Chair.*

### I.—OBSERVATIONS ON THE STATE OF THE PUPIL IN CHEYNE-STOKES RESPIRATION.

By DR. FINLAYSON.

THE remarkable series of ascending and descending respirations known under this name do not require description, as they are now so well known, and have been so often graphically represented in tracings of the chest movements. But some authorities have drawn a distinction between what they call typical Cheyne-Stokes breathing and the irregular or sighing respiration of cerebral disease. I wish to make it clear that in my opinion this difference is only one of degree. No doubt, as a rule, the Cheyne-Stokes breathing, as found in a case of cardiac or renal disease, is *usually* much more "regular in its irregularity" than similar disturbances of the respiration in brain cases; but we see at times the most perfect types of Cheyne-Stokes respiration in, say, a case of tubercular meningitis, and all gradations between the two forms are met

with—the first being at times less regular, and the other (or cerebral) form more regular than usual, or even, as I have said, attaining the most perfect and typical form which could be imagined. It is necessary to explain that I include this cerebral form under Cheyne-Stokes respiration, otherwise it might be said that some of my cases, in which I had observed changes in the pupil, were not cases of Cheyne-Stokes breathing at all. The distinction of the cerebral respiration from typical Cheyne-Stokes breathing has been insisted on by Biot in his memoir on the subject and by others; but in favour of the view which I have so confidently announced I can quote the authority of the late Dr. Hilton Fagge, and other eminent observers.

It has long been known that in Cheyne-Stokes breathing the pupil undergoes changes, and this fact is referred to by Biot and others. The pupil almost always becomes contracted during the period of apnœa, so as to resemble its state during sleep, to which the period of apnœa has in some other respects a certain resemblance. The patient often, indeed, seems as if sleeping, his intelligence seems for the moment obscured, and he often utters slight groaning sounds like one dropping over into an uneasy sleep. With the first of the ascending series of respirations the pupil slightly dilates, and then rapidly enlarges during the full deep breaths which follow. I have often watched these changes, and have always noticed the relationship, when it existed, to be as described: the pupil contracted during the period of apnœa and dilated during the full deep respirations. Sometimes the alterations in the pupil are but slight, and I should state that the reversed relationship has been described by some writers (*viz.*, dilatation during the period of apnœa), but I have never seen this myself in any case.

But what I wish to call attention to is quite another alteration in the pupils, to which I have not found any very distinct reference in the observations of others—*viz.*, a *rhythmical contraction and dilatation with each individual breath* in the full deep series of respirations, the pupil dilating with the inspiration and contracting with the expiration. The first case which called my attention to this peculiarity was not a case of Cheyne-Stokes respiration at all. It was one of stertorous breathing in prolonged coma, occurring in a dropsical girl with uræmic convulsions. It was associated possibly with some slight cerebral lesion also, as there was partial temporary hemiplegia after she regained consciousness. In her case, during the full, deep, stertorous respirations, the

pupils contracted and dilated in a regular manner, each inspiration being associated with an enlargement, and each expiration with a contraction of the pupil. The phenomenon was so striking as to lead me to watch the next few cases of Cheyne-Stokes breathing, and in several cases a similar rhythmical alternation in the size of the pupil was seen in the period of full, deep respirations. One of the most striking was in the case of a child who died very early in the course of a tubercular meningitis, as ascertained by *post-mortem* examination. In his case, the rhythm of the Cheyne-Stokes respiration was perfect, and the period of apnoea so marked as to suggest several times the idea of his being actually dead till the ascending series of respirations again set in. In various other cases of Cheyne-Stokes respiration in the adult, occurring in its most typical form, I have witnessed the rhythmical changes with each respiration, although in some cases it has been absent and in some subjects the pupillary changes of any kind are slight. I do not think I can give the *post-mortem* changes in these cases of Cheyne-Stokes respiration, as some of them were only seen in consultation or incidentally, but I may say that they presented the clinical features with which, in my experience, Cheyne-Stokes respiration is most often associated—viz., a combination of cardiac and renal symptoms.

It is possible that the alternation of dilatation and contraction of the pupil during the inspiration and expiration, to which I have referred, may be but an exaggeration of a physiological variation which has been alleged to occur in the pupil with every breath. I have not studied the matter sufficiently to be able to satisfy myself on this point, although I have thought I could sometimes catch such a change in the healthy pupil.

I do not venture into the intricate question of the cause of Cheyne-Stokes breathing, or of the concurrent changes in the pupil, but I have thought my observations might interest the members of the Society as a mere matter of clinical observation.

*Dr. Alexander Robertson* said that the pupil had the most remarkable range of association with the central nervous system. Any powerful sensory impression was capable of producing dilatation of the pupil. Even muscular action might be accompanied by dilatation. In the condition of health this remarkable range of association between the centres which control the pupil and the higher centres of the brain could be verified. Several years ago he, like Dr.

Finlayson, made careful observations on the condition of the pupil as observed in various diseased conditions, including Cheyne-Stokes respiration. In this condition he noticed particularly the changes of the pupil through the respiratory cycle—in the state of apnoea being reduced to a mere point, and again dilated at the acme of the cycle. In one case, along with the dilatation of the pupil, the contraction of the muscles was also extreme, the muscles relaxing as the breathing went down. In one case the *post-mortem* examination disclosed two sanguineous effusions in the anterior part of the brain; but it was not easy to see that there was any connection between this and the symptoms.

Dr. M'Vail said that in his ward in the Western Infirmary there was at present a case of Cheyne-Stokes respiration in which the changes of the pupil could be watched, and they were extremely marked. But the *most notable circumstance in that case was that on the first day of Cheyne-Stokes respiration the phenomena were reversed*. Instead of the contraction in apnoea, it took place during breathing, while dilatation occurred at the other side of the cycle. This he pointed out at the time to students as a remarkable reversal of the ordinary condition.

Dr. Morton said that the late Dr. Paxton, of Kilmarnock, possessed a remarkable voluntary power over the iris, contracting and dilating his pupils at pleasure. In studying these cases the possibility of the patient possessing such a power must be taken into account.

Dr. Finlayson said that Biot doubted whether such a reversal of the symptoms as Dr. M'Vail had observed ever took place. He was aware that it had been averred that this reversal had been observed, and therefore he had been on the watch for many years, but he had never seen such a thing. However, he did not doubt the correctness of Dr. M'Vail's observation. In regard to Dr. Paxton's case he might state that on further investigation it turned out that Dr. Paxton merely possessed in an exaggerated degree a power which everybody possessed—viz., that of moving his pupils in association with convergence. He could not control his pupils apart from the motion for accommodation.

DR. D. C. M'VAIL read a paper, dealing with the loud, rough, thrilling murmur that precedes and runs up to the first sound. A short historical account was given of the views entertained by different physicians with regard to the causation and significance of the murmur. The paper argued

that this, the loudest and strongest by far of all cardiac murmurs, could not possibly be caused by the *auricular* contraction, but must necessarily be owing to *ventricular* contraction. The author of the paper held that there is always, when the ventricle under normal conditions contracts, a regurgitation of blood into the auricle—a regurgitation prior to, and ending with, the complete closure of the auriculo-ventricular valves. This “physiological regurgitation” is necessarily of considerable amount, and is under normal circumstances accomplished without audible sound. But under certain abnormal conditions it becomes audible, and often loudly so. The conditions requisite for this are those that give rise to “fluid veins” of backward direction at the auriculo-ventricular orifice. These conditions are three in number. 1st. There must be blood in the auricle; 2nd. This auricular blood must be under some degree of tension at the instant when the ventricle begins to contract; and 3rd. The ventricle must force a stream of blood *into* the mass of the auricular blood. The friction between this *core* stream and the enveloping and—for the moment—practically stationary auricular mass of blood gives rise to the murmur. When the heart is quite normal there is no audible murmur, because the regurgitant or *core* blood from the ventricle finds in the auricle either no blood or an amount of blood quite insufficient to develop audible frictional vibrations. But when there is stenosis of the auriculo-ventricular valve, of sufficient amount to prevent anything like complete emptying of the auricular cavity prior to the beginning of the ventricular systole, then there is blood in the auricle sufficient to develop audible vibration by friction with the regurgitant ventricular blood. There is not in all cases of auriculo-ventricular stenosis, nor at all times an audible murmur; this is due to one or other of two circumstances—1st, the auricle for the time being is, notwithstanding the stenosis, able to empty itself to such degree as to leave within itself a quantity of blood insufficient for audible frictional vibration with the regurgitant blood; or, 2nd, the auricular wall is so flaccid and dilatable at the beginning of the ventricular contraction that the regurgitant blood easily finds room for itself in the auricle by pushing before it—*en masse* as it were—the auricular blood; the regurgitant blood not requiring to *penetrate into* the mass of auricular blood. The auricular blood is not under such degree of tension as to resist sufficiently the stream of regurgitant blood, so that a fluid vein could take place within it. In this way the author accounted for pathological specimens that



he exhibited of great mitral stenosis where, although, under careful observation for weeks prior to death no murmur running up to the first sound had been heard. He altogether objected to the murmur being named *pre-systolic* as Gendrin, in 1841, had named it, or *auriculo-systolic* as Gairdner had called it in 1861. It is altogether *ventriculo-systolic*—differing from the murmur at present known as mitral ventriculo-systolic only in that it *precedes* the acme of mitral valve closure. In practice it is, as every one knows frequently *continuous* with the murmur ordinarily designated mitral regurgitation—the regurgitation *after* the acme of mitral valve apposition. Were the murmur really *auriculo-systolic*, then it could not possibly without interval be continuous with the murmur that is universally admitted to be ventriculo-systolic—an interval during reversal of blood currents there would necessarily be, quite appreciable to the ear—which easily recognises an interval so short as the  $\frac{1}{100}$ th of a second. The author held that all intra-cardiac murmurs are due to “fluid veins,” it being impossible that any valve or cord, or membranous or muscular projection into the heart’s cavity, could, in a fluid so dense as blood, vibrate with sufficient freedom and rapidity to give rise to audible sounds. In the course of the paper, frequent reference was made to the views and writings, particularly of Gairdner, Barclay, Flint, and Balfour.

Dr. Gairdner said that he regretted having been unable to hear the first part of Dr. M’Vail’s communication; but having, by the courtesy of the author, been furnished with an abstract of it, he was in a position to offer some remarks upon the whole argument, as well as on that portion of it which he had the advantage of hearing in detail. He would begin by admitting that he did not feel any great call to deal with the mere theoretical questions discussed as to whether a fluid vein, and a murmur associated therewith, could or could not arise under certain circumstances presumed to exist in connection with the so-called murmur of mitral stenosis. Many years ago the late Dr. Barclay, of St. George’s Hospital, had started the view that this murmur was one really of regurgitation; and he (Dr. Gairdner) had at the time exactly the same impression as now, that the merely speculative considerations involved in this theory could only distract the attention, and serve to obscure the points which were of real practical importance. The distinctive rhythm of the murmur, its characters as bearing on diagnosis, and its association with the lesion of the valvular orifice were, after all, the facts on which the argument must turn; and he could not allow these positive facts to be brought into apparent controversy on the ground of a remote

presumption that a mode of action of the valve involving "physiological regurgitation" (which, after all, was only an assumption) was the barely possible cause of a murmur the whole of the clinical characters of which pointed to quite another mode of origin. It was remarkable that, with the exception of the late Dr. Austin Flint, no one of any authority had ever attempted to show that the characteristic pre-systolic, or, as he (Dr. Gairdner) preferred to call it, auricular-systolic murmur could occur in the *absence* of mitral stenosis. Now, Dr. Flint's name was a venerable one in medicine, and Dr. M'Vail was fully justified in referring to him, and in treating his views with the utmost respect; but not in claiming Dr. Flint's experience as opposed in general to the more commonly received theory of the murmur. Dr. Flint, so far from disputing the auricular-systolic view of the murmur, maintained it most completely, and admitted that the exceptions to the coincidence of the murmur with narrowing of the opening were extremely rare, and certainly did not involve the unsettling of the foundations previously laid as regards the great majority of cases. How rare these exceptions were (even admitting them to exist) might be judged of from the fact that Dr. Flint's original statement to this effect was published in 1862; and in the last edition of Dr. Flint's work on the *Practice of Medicine*, posthumously edited by his son, there was a footnote giving only one corroborative instance as having appeared in an American journal during that long interval. Moreover, Dr. Flint's observations, though they might easily have escaped notice in the first instance in this country, were certainly well known to him (Dr. Gairdner) ever since the discussion between Dr. Barclay, of London, and Dr. Balfour, who had particularly discussed and opposed Dr. Flint's view. For a long series of years, therefore, he (Dr. Gairdner) had been carefully on the watch, and it was not until just the other day that he had met with a single instance even apparently corroborative of Dr. Flint's curious theory that the pre-systolic murmur might, under very exceptional circumstances, be determined indirectly by a lesion of the aortic valves. Whatever, therefore, might come of Dr. Flint's peculiar theory, it could not be made available to support the views advanced by Dr. M'Vail. As regards the production of the auricular-systolic murmur by a regurgitant movement of the blood towards or into the auricle, it was, perhaps, most fitting to say that the arguments adduced on the present occasion were quite insufficient to support it. Be it remembered that this murmur was not, under ordinary circumstances, one at all difficult to distinguish either from the equally well known murmur of mitral regurgitation (commonly

so-called), or from the aortic murmurs either of obstruction or of regurgitation. It may even occur in association with any of these, and yet be distinguishable from them by an experienced ear. Now, when this peculiar and very characteristic murmur concurs in point of time, as it usually does, with the auricular systole only, it is certainly right to call it *auricular-systolic*, and this, without necessarily adopting any theory as to its being caused only by the contraction of the auricle; and yet the presumption in favour of the theory that it arises from the auricle forcing a diminished current of blood through an obstructed auriculo-ventricular opening will be much stronger (even apart from actual observation) than that it arises in any other way. But the actual facts observed show that this murmur, when accurately distinguished, is found in association only with mitral stenosis, or with obstruction of some kind to the flow of blood from the auricle into the ventricle. The all but unique case which Dr. M'Vail referred to as one to be explained in accordance with his own views, really did not, according to Dr. Gairdner's observation of it, support these views at all. The theory submitted that evening was that the so-called auricular-systolic murmur, preceding, as it did, the first sound of the heart, was produced during a momentary regurgitation occurring before the completed closure of the auriculo-ventricular valves. But in that case the murmur, as very carefully observed and described at the time, was not momentary, nor was it confined to the interval immediately preceding the first sound. It was a long-continued and variously-inflected murmur, covering the entire interval extending from the second sound, through the pause or rest, and then quite up to the first sound, which indicated the commencement of the new systole. Such a murmur could not, under any conceivable circumstances, have been caused by regurgitation, seeing that the ventricle during the whole or the greater part of this interval was in process of dilatation, and not contracting at all. But there was yet another difficulty in making this case fit in with Dr. M'Vail's hypothesis. It was admitted that in the normal state of parts the regurgitation which was supposed to exist, physiologically, at the very commencement of the ventricular systole, was not attended by murmur. Well, in the case referred to by the author of the paper, there was just such a normal state of the parts as regards everything which bore upon the closure of the valves, and the condition of the ventricles. The obstruction in this case was from a tumour loosely depending from the lining membrane of the auricles, which, during their systole, or even when the current

of blood was directly from the auricles into the ventricles, was pressed down towards the opening after the manner of a ball-valve; but whenever the *vis a tergo* was withdrawn, still more when the impulse from the ventricles was in the direction of regurgitation, this tumour would necessarily be lifted up from the opening, and leave it free, with a normally acting valve. There was, accordingly, no reason at all why there should have been a murmur of regurgitation in this case, any more than in that of a normally acting heart. As to the theory of "physiological regurgitation," Dr. Gairdner hesitated in accepting it, at least in any practical sense. There might be, for aught he knew, a kind of regurgitation, for an indefinable instant, even at the valve of a steam engine. But no one maintained, in the case of the aortic valve, for example, that a prolonged and definite murmur of regurgitation can arise in this manner, and it was not at all clear that it was so in the case of the mitral, or even of the tricuspid valve. Many years ago, he had been led to investigate the mechanism of these valves, and he came to the conclusion that, although different in kind, the mechanism of closure was, normally, quite as perfect in them as in the case of the sigmoid valves. But in any case the whole of his clinical experience led him without any reserve to affirm the murmur of regurgitation (v. s.) to be one thing; and the murmur of obstruction (A. s.) in the auriculo-ventricular orifices to be another and a quite distinct thing. So much, he thought, had been made clear beyond dispute, upon the basis of clinical observations repeated in many places by many different men, although there were not wanting even recently published text-books in which the matter was considerably muddled up. But to admit, on the basis of an unproved hypothesis, the conclusions to which Dr. M'Vail had come to-night, would be the bringing back of chaos. In dealing with this subject, accordingly, it was necessary to subordinate theoretical considerations to facts which were capable of demonstration, and of the utmost importance in diagnosis.

Dr. M'Vail regretted that, owing to the lateness of the hour, it was impossible on that evening to make any reply to the most interesting, able, and important defence by Dr. Gairdner of the doctrine advanced by him in 1861. But he (Dr. M'Vail) held that the matter could not remain in its present position, as the *post-mortem* room is continually showing how groundless is the assumption that the murmur under consideration is due to vibrations caused by blood passing into the ventricle through a stenosed auriculo-ventricular orifice.

GLASGOW OBSTETRICAL AND GYNÆCOLOGICAL  
SOCIETY.

SESSION 1886-87.

MEETING VII.—27TH APRIL, 1887.

DR. SLOAN, *President, in the Chair.*

MR. C. W. SMITH, Halifax, and Mr. David N. Knox were duly admitted to the Fellowship.

## I.—APOPLEXY OF THE PLACENTA.

BY DR. P. CALDWELL SMITH, of Motherwell.

The placenta was shown. Mrs. B., æt. 42, had a child at full time in 1881. In August 1885 she had a premature birth at 5½ months, the placenta showing extensive signs of fatty degeneration. The recovery was complete. In March of this year she was again seized with labour prematurely—viz., at the 6½ months. When called, he found she had fainted, and it was evident severe blood loss had taken place, but there was no external manifestation of it. There had been a large escape of watery fluid which the lady thought due to the usual escape of amniotic fluid, but on examination the membranes were found to be entire. The placenta was expelled about five minutes after the foetus, and after it several clots as large as itself were expelled. Placenta was only healthy in about one-third of its area, the rest showing large hemorrhages into its stroma, these being of recent date. The lady died in five days of uræmia following an acute attack of Bright's disease. He considered the *ante-partum* fluid discharges to have been due to compression of the blood clots between the uterine and placental surfaces. There had been no oedema of any part, nor any arterial tension, but the pulse ranged from 130 to 140.

## II.—CÆSAREAN SECTION.

The discussion on Mr. Nairne's paper on this subject was opened by

Mr. Thomas F. Gilmour, who said that he thought the language of the paper was too hot, too sweeping, and too hortatory. It was intemperate, and therefore unscientific, to use such phrases as "to kill a child by selection," "improved system of hanging," or "infernal murderous operations;" or again, "I speak out for the child, and shall never be a willing

partner to such slaughter." The poetry and the digression on statistical fallacies, and the reflexions and resolves, were in a Carlyleian vein, and were scarcely called for on such a subject to such an audience. This was a matter in which Lawson Tait was a great sinner, and disciples usually found it easier to imitate the vices of their leaders than their excellences. It led to heat and friction, and these always hindered scientific progress, and sometimes diverted its course entirely.

He thought that if there was one operation in obstetric practice which illustrated and proved the Hippocratic aphorism that "the occasion is fleeting, experience fallacious, and judgment difficult," it was the Cæsarean Section, for, in spite of its great antiquity, and in spite of the fact that parturition is the only constant and essential medical incident of all generations, and all states of civilisation—for if things subsisted births must proceed—we have a long and interesting list of quotations and judgments nearly all against the operation given by Mr. Nairne himself, from men who had had experience of it, and whose judgment we are bound to respect. These men could not have turned thankfully to craniotomy without good reason; they must have chosen it as the least of inevitable evils after being brought face to face with the alternative, and while any of us might choose a different procedure, we *dare* not, he contended, claim that our so doing was from a higher or a nobler motive. Cæsarean section, like other operative procedure, must be justified or condemned by the results obtained.

Albeit he admitted that an operation to supersede craniotomy was required; though it would be still better if we could supersede the necessity for it by forbidding or preventing such pregnancies as must end that way. This would be the true remedy. People were not allowed to hawk laces, to sweep chimneys, to drive a cab, or carry luggage, or navigate a ferry-boat without being examined as to their fitness, and licensed as a proof of their fitness, and he maintained that examination and license to marry would be a prudent thing in every civilised state, and that it was a part of the duty of medical men to advocate such action by the state. But, taking things as they are and until they become better, he thought that where a living child cannot be born *per vias naturales* much can be said in favour of opening the mother's abdomen instead of the child's head. Though it was questionable whether in such instances the child was likely to be a valuable addition to the community. The laws of heredity forbid it. Figs are not gathered off thistles, and we were not likely to get well formed or prudent children from badly formed and specially

imprudent parents. He thought Mr. Nairne rash in saying he would never again perform craniotomy. All the same he admired him for taking a clear and resolute view of his duty, and acting up to it. He thought Porro's operation superior, as it ensured the patient's sterility in future. No operation is complete or commendable which leaves the patient liable to the same dangers again and again. There are cases on record, for instance, where the Cæsarean section has been performed on the same patient four times. It is thus a form of meddlesome midwifery—if we meddle at all it should be once and for all.

Regarding the case of Mrs. W., he thought it could hardly be profitably discussed in the absence of full details of diagnosis, operation, and after treatment, and especially of an autopsy. There could be no doubt, however, but that the death was due to preventible causes, and he humbly ventured to think that chief among these was the performance of Cæsarean section! A person who, however deformed, had passed safely through the ordeal of pregnancy eight times already, who had reached the age of 40 years, and was unlikely ever again to trouble the obstetrical physician, and who was first seen at or during the 7th month, would have been best left to complete her generative history by methods which her previous experience had sanctioned. In conclusion, and referring to the position of the foetus, he said he only knew of one position of the unborn foetus which could ever become a determining factor in the choice of an operation, and that was its being extra-uterine.

Dr. Murdoch Cameron thought it was quite possible to deliver *per vias naturales* with a conjugate diameter of even less than 2 inches, and, therefore, that an attempt should have been made to deliver Mrs. W. by means of version and forceps, and failing them, craniotomy. The life of an infant unborn was not to be put in the same scale with that of the mother. Dr. Cameron instanced two cases where he had succeeded in delivery thus, and showed one of the women with her child. The mother was quite dwarfish, with a pelvic conjugate under  $2\frac{1}{2}$  inches, and the left oval oblique deformity.

Dr. W. L. Reid thought all were agreed as to the advisability of avoiding Cæsarean section, if possible. Craniotomy was a safe operation for the mother in all cases except those whose pelvic conjugate was only  $1\frac{1}{2}$  inches or under. He would prefer Porro's operation to the Cæsarean section if a favourable case presented itself.

Dr. Oliphant referred to the advantages of section of the pubic rami as advocated by Dr. Macewen.

Dr. J. K. Kelly thought these operations were rarely if ever required, and that the long, straight forceps (Glasgow pattern)

could effect delivery in cases where, ordinarily, craniotomy was performed. Failing this, he was inclined to favour Macewen's operation of section of the pubic rami.

*Dr. N. Stark* wanted to know why exact measurements were not taken, and apprehended that there was a disparity of observation between the medical men. He thought a mistake had been made in performing Cæsarean section in Mrs. W.'s case. He criticised the after treatment, and thought the nurse had had too great discretion left to her.

*Dr. J. Adams* said that after the sweeping condemnation which had been passed by previous speakers upon the operation he felt inclined, like Shylock, to rise and admit, "with bated breath and whispering humbleness," that he had performed Cæsarean section at any time. He thought the thanks of the Society undoubtedly due to Mr. Nairne for introducing the subject to their attention, though, probably, his views would have had more weight had they been put forward in a less hysterical style. Possibly previous speakers had spent too much time in dissecting his composition and his case, so he would confine his attention to the Cæsarean section, and drop personal considerations.

It might be well that he should first give an outline of the case he himself had operated on just three years ago.

The patient was a dwarf, 4 feet in height, aged 23, and married about 7 months. Her physician, Mr. Grieve, had only seen her for the first time after she had been 12 hours in labour. She had lateral and posterior spinal curvature, and some three years previous to her marriage, Dr. Macewen had operated on both her legs for knock knee. No os uteri nor presentation could be made out, and the finger could hardly pass between the sacral promontory and the pubis. In fact the available space was barely one inch.

She was put under chloroform at 7.30 P.M.; her abdomen was carefully washed with a 1 to 20 solution of carbolic acid and I proceeded to operate with the assistance of Mr. Grieve, Mr. Robert Millan, and my brother, Mr. Fred. Adams. An incision was made from the umbilicus to a point  $1\frac{1}{2}$  inches above the pubis. This had subsequently to be continued upwards about 2 inches, and to the left of the navel. All bleeding points were easily controlled, and the peritoneal cavity opened without difficulty. Warm moist towels were now placed around the wound and over the uterus which came boldly into view having no bowel in front of it. A bold vertical incision was now made into the uterus, and the blood which escaped in considerable quantities was rapidly removed by sponges, while the assistants kept the abdominal



parietes in close contact with the uterine surface. The placenta was adherent to the anterior uterine wall, and was rapidly detached and removed, and the child, which lay in first cephalic position, was grasped by the breech and extracted. The uterine wound closed firmly round the neck of the child, and delayed complete delivery. After all vestiges of the membranes and liq. amnii had been removed, the os was found dilated to the size of a half-crown piece. Prior to administration of the chlorform, one fluid drachm of the liquor ergotæ had been administered in whiskey, and so the uterus remained in the pelvis contracted like a cricket ball. There was no threatening of hæmorrhage. No sutures were used and no drainage tube, as he thought the first would not hold, and the latter would act more as a plug than a drain owing to its liability to get blocked by a clot. The abdominal wound was closed all but an inch at the bottom by chromic acid catgut. Protective with gauze dressings were applied. A binder of flannel was also wrapped round the trunk. She came rapidly and easily out of the chloroform, and lay quite happy watching her little one whose presence caused her much joy. Her voice was strong, her pulse was good, and she was very anxious to obtain solid food. This condition continued till 11.15 P.M. when she suddenly died. The uterus had never relaxed its contracted condition, and *post-mortem* no blood was found in the abdominal cavity. There had been but little discharge per vaginam, and only slight blood clot in the uterine cavity. No examination of the head and thorax was permitted.

Dr. Cameron had overlooked the important fact that the question of safety of an operation is largely affected by the experience of the operator, and thus he felt that enbryotomy was quite as dangerous in the hands of an unskilled obstetric surgeon in very contracted pelvis as the Cæsarean section would be in the hands of a highly skilled surgeon. Evisceration was a long tedious operation, fatiguing to the surgeon, and most exhausting to the patient, and this latter fact largely affects the chances of recovery. The certainty of septic conditions arising from bruising, wounding, or lacerating the vaginal passages was considerable.

Dr. Stark had indicated his preference for Porro's operation, but to those who have had any experience of abdominal surgery, or who have dissected the pregnant uterus at full time, it will be apparent that to talk of applying a clamp round the cervix and remove the uterus is easier said than done. Apart from the difficulty of dealing with the very much enlarged broad ligaments, the size of the stump would

leave great opportunities for hæmorrhage and septic infection. In the event of Cæsarean section becoming more successful with greater attention to points of detail, the ovaries could be removed at a later period without much danger. He therefore objected to Porro on account of the large raw vascular surface left, and on account of the shock involved in removing important organs like the uterus and ovaries. He believed that with the experience of many cases and many operations the Cæsarian section would have a much more successful issue in future, and he begged to indicate the following important points to be borne in mind in dealing with it:—The incision should be kept to the vicinity of the fundus, as dissection plainly shows that at the cervix the blood-vessels are large and numerous, and there is a circular ring of muscle which would keep the wound gaping and tear through stitches if they were employed. He saw no cause to regret not having used stitches in the case narrated, but in another case he would certainly follow the plan indicated by Dr. Samuel Sloan, and put a large number of stitches in the wound. To be effective they should be in three sets—viz., one through mucous membrane, one through muscular tissue, and one—most important—a large number through the peritoneal edges. These edges should be slightly inverted to cause adherence of the opposed surfaces. There is little fear of septic matter travelling *via* mucous membrane or muscle tissue, but the entrance of the lochia into the peritoneum was much to be dreaded.

In regard to the relative value of the two lives, he thought few would like to take Mr. Nairne's view that they were exactly equal, and he must admit that the mother demands the chief consideration; but, on the other hand, he could not agree altogether with Mr. Gilmour's contention that a child of ricketty parents is *per se* probably a useless life, and better never to reach maturity, and that this is another argument in favour of embryotomy. Many of our greatest men have been deformed, ricketty, or crippled, and yet have done vast good to the human race.

Dr. Robert Pollok said that he had seen the case of Mrs. W. for the first time when called to assist at the operation. It was not to be gainsaid that the operation was called for in some cases, but its overwhelming danger was not to be overlooked. We can't throw to the winds all the wisdom of our teachers. He turned Longfellow's verse against Mr. Nairne, as it was the patient in this case who bled, &c. He criticised the absence of exact measurements. It might be regarded as certain, however, that the conjugate was  $2\frac{1}{2}$  inches, and that

being so, he thought premature labour could easily have been brought on at the 7th month with safety to the mother and infant. He thought there was great disparity in the value of the respective lives of the mother and infant.

*Dr. Richmond* asked if it had not occurred to *Mr. Nairne* to induce labour at the 7th month?

*Mr. Skene Keith* remarked that he did not think it was a matter of great importance what the exact diameters were in the case of *Mrs. W.* She wanted to have a live child, and he did not see why an attempt to gratify her wish should not have been made. He thought a great deal depended on the operator. With reference to *Mr. Nairne's* tabulated summary, he thought the first proposition was too exclusive, as elytr laparotomy offered an alternative to the section and was preferable to a Porro. In elytr laparotomy there was no injury to the peritoneum; and there was not an ordinary stump, as the incision went down through the broad ligament. Neither would he go so far as the second proposition. In doubtfully bad cases he would prefer craniotomy, as the mother was our first consideration. The third proposition was too dogmatic, and as to the fourth, if we knew that a viable child was not capable of being born, it was needless to use the long forceps. The operation was quite justifiable in suitable cases.

*Dr. Marshall*, Coatbridge, thought *Mr. Nairne* had done a good service in raising the question, though he might have done better had it been on another case. He could not sympathise with him in leaving the case till the 9th month. Premature labour should certainly have been induced when the patient first came under notice. He wondered also why, when he did decide to operate, elytr laparotomy had not occurred to him. Full weight should be given to the lessons of statistics, and he did not think there could be any comparison between the value of the life of a mother and a child, especially one yet unborn.

*Dr. Malcolm Black* narrated a case where *Dr. George Buchanan* had been the operator. The conjugate osseous measurement was  $2\frac{1}{4}$  inches. The patient was in good general health, not exhausted in any way, and no attempt had been made to deliver her by forceps. At the *post-mortem* blood was found in the abdomen, and the stitches had given way in the uterine wound. He criticised the essayist's views about craniotomy and the amount of danger accruing from it to the mother, and repudiated them. He also questioned the advice given to *Mrs. W.* at the beginning. For stitching the uterine wound he advocated silk sutures boiled first in solution of the perchloride of mercury.

Dr. Park stated that he should not have sanctioned the Cæsarean section in Mrs. W.'s case had the pregnancy not been so far advanced ( $8\frac{1}{2}$  months) when he first saw her. The facts also that the presentation was cross and the cephalic extremity lying in the right or narrower side of the pelvis, and that the amniotic fluid was scanty, weighed with him. He pointed out, in answer to Mr. Gilmour, that any attempt to have turned would have involved a complete version and transposition of the foetal head to the left side of the pelvis—the only side through which it might have been conceived possible to drag the head. Doubtless it would have been easy to perform podalic version, but it would have landed the accoucheur in tremendous difficulty with the aftercoming head. If the case had presented itself to him at the 7th month, he certainly thought he would have induced premature labour; but as the woman wished to have a living child, he sympathised with Mr. Nairne in his ambition to gratify this longing. He had found, by reference to Neale's *Digest*, that many cases of recovery after Cæsarean section had been reported—some where the patients operated on themselves. There was nothing, therefore, inherent in the operation itself to make it unsuccessful. Mr. Nairne had omitted to state that the patient had had a hypodermic injection of morphia prior to being placed under chloroform, and that she had thus been enabled to do with less chloroform, and had obtained a prolonged and beneficent sleep after the operation. Indeed she continued in a most favourable condition till the morning of the third day, when she had an enema for the purpose of inducing action of the bowels. As it did not operate, the nurse took it upon herself to repeat the injection to the extent of two pints, all of which was retained, and gave rise to considerable distress. In addition she had passed that night without either draught or hypodermic injection. Dr. Park tried to relieve the abdominal distention, and to draw off some of the imprisoned enema by means of O'Beirne's tube, passed about 20 inches up into the colon, but without effect, only an ounce or two of fluid coming through. In fact, obstruction of the bowels was the first beginning of trouble; but it was rapidly followed, if not preceded, by peritonitis, which ran an acute course until death ensued. With reference to this complication, he referred to the very full and exhaustive consideration it had received from the German Gynecological Society, as reported in their *Transactions* and reviewed in the current number of the *International Journal of Medical Sciences*.

The President expressed his admiration of Mr. Nairne's paper none the less heartily that he differed entirely from the

conclusions Mr. Nairne had felt it right to come to. The language of the paper did not certainly partake of the utterance of the "still small voice" which Mr. Gilmour would have preferred, but he felt that the style adopted by Mr. Nairne was justified as an expression of his own strong feelings. The pathos of the sentence, "She saw her baby and was glad," was very touching. He objected, however, to the argument of the essayist—namely, that because the life of the child was valuable we were bound, in order to save it, to perform such an operation on the mother as would almost certainly kill her. No husband and father would for a moment consent to have the life of the wife and mother treated as no more precious than that of the unborn babe. He confessed, himself, however, as very anxious to get rid of so horrible an operation as craniotomy; but he thought that if the blame of this operation rested on any one it was more on the surgeon than on the gynecologist, for as soon as the former could show even a very few successful cases in Glasgow, the latter would most gladly lay aside the perforator. He would advise, especially in view of recent improvements in the operation of Cæsarean section, that where the antero-posterior diameter did not exceed 2 inches—the child being presumably of average size—the woman should be recommended to submit to the operation, since her chances were not much worse than with craniotomy. His experience of Cæsarean section was that few points were of more importance than the stitching of the uterine wound, the muscular walls first and the peritoneal edges after. He referred to an article in the *American Journal of Obstetrics* for October, 1886, where many, in his opinion, valuable improvements in the operation were recorded. He objected strongly to the insertion of a drainage tube from the uterine cavity into the vagina, believing that this hindered rather than helped the passage of clots.

Mr. Nairne, in reply, stated that when he read his paper, he had no expectation that it would have a favourable reception. It would be impossible for him to answer in detail all the criticisms that had been passed upon it. He could not admit that any further consultation was necessary prior to the operation. The opinions of the leading consultants were well known upon the subject. He had dismissed the question of the induction of premature labour, as in her previous miscarriage the difficulty experienced in delivery was very great. He felt disposed to give Mr. Skene Keith's advocacy of laparo-elytrotomy a favourable consideration.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

## EPIDEMIOLOGY.

By JAMES W. ALLAN, M.B.

**Cholera** is reported in Italy, Sicily, Sardinia, and Malta, and a "suspicious" disease in Spain.

**Cholera.**—"Report of a Localised Outbreak of Cholera, due apparently to Contaminated Water and Milk. By W. J. Simpson, M.D., Health Officer of Calcutta." (*Indian Medical Gazette*, May, 1887.) The story of this interesting outbreak of cholera, which was very carefully investigated by Dr. Simpson, may be condensed as follows:—

The *Ardenclotha* from Hamburg, with salt, arrived in Calcutta on 25th February. Dr. Simpson, learning that cholera cases had been sent to hospital from a ship, thought the opportunity a good one for ascertaining the etiology of the disease. Accompanied by Dr. Lynch, the medical officer for the port, he went on board and found that there had been five cases of cholera, besides cases of diarrhoea in the crew. "Climatic influences," "sewer gas," "foul underground air," the condition of the ship, the water in the ship, and the food in the ship were all dismissed. None of them afforded any clue to the origin of the disease. The movements of the sailors on shore, although carefully investigated, proved equally barren of result. But it was found that milk was obtained from "a native who visited the ship daily." This native was also a washerman. It was found that "of the 10 men who drank milk, 9, or 90 per cent, were affected, 5 with diarrhoea and 4 with cholera. The sailor who partook of the milk and escaped suffering from any ill effects describes the amount he had as a thimbleful, meaning that he had a very small quantity. Of the 14 men who did not drink milk, 13, including the 8 officers who used preserved milk, were unaffected; 1, however, or 7 per cent of the total, was attacked with cholera. There is this to be said, however, that he was the last attacked, and it is quite possible his cholera was of a secondary nature or from an independent source. Again, it is at most doubtful whether he did not touch the milk. However, assuming that the last case could not be explained by milk influence, it does not affect the fact that of the milk drinkers, 5, or 55 per cent, were affected with diarrhoea, and 4 or 44 per cent with cholera; and from the foregoing facts it seemed probable that the outbreak was traceable to the milk."

The residence of the milk seller was next examined. He lived in a hut on the other side of the river at a place called Howrah. Two small tanks were situated near by, in which his family washed and bathed. The solitary cow was in good condition. The milkman's custom was to mix his cow's milk with some procured from the bazaar and add "a sufficiency of water (the usual quantity is  $\frac{1}{4}$  seer of water to  $\frac{3}{4}$  seer of milk)."

It was ascertained that there was no cholera in the milkman's family, but "there had been recently several cases amongst his neighbours." The suspicion that those neighbours might have got cholera infection from the bazaar milk was "negatived." It was found that the cholera had been brought to Howrah by the conveyance thither of a cholera patient on the 2nd or 3rd of March. "For the next few days, until his death on 10th March, the drainage from the hut had added to it the dejections of the patient; the soiled clothes were also washed in the tank. On the 7th and following days—i. e., a few days after the coming of the cholera-stricken patient to the neighbourhood, the inmates of the large house who used the tank were affected with cholera."

Now for the state of matters on the *Ardenculha*:—

"On the morning and during the day of 8th March, 4 cases of diarrhoea.

Morning of 9th, . . . . . 1 case of cholera.

10th, . . . . . 4 "

(One of which is doubtful whether drank milk.)"

Milk stopped on 10th."

Supposing the milkman's denial of using the tank tainted with cholera to be true, Dr. Simpson urges that the one tank might taint the other, as they were in close proximity, and "the soil between them is of a very open and porous nature."

Dr. Simpson contends that it is "in the highest degree probable" that the cases at Howrah were due to cholera-tainted water, and those on the *Ardenculha* to milk diluted with cholera-tainted water.

**Hydrophobia.** (*Indian Medical Gazette*, April, 1887.)—"A Case of Hydrophobia."—Surgeon-Major P. J. Freyer, M.D., of the Indian Medical Service, gives the details of an interesting case of hydrophobia which came under his care on 27th September. The patient was a cavalry officer who was supposed to be suffering from "fever." When the doctor saw him he was perspiring heavily, and was supposed to be in the "sweating stage" of the fever. But it was noted as "peculiar" that the patient started up from the recumbent posture, and complained of a feeling "as if the upper part of his chest was being compressed." Dr. Freyer ordered "the usual remedies for intermittent fever." The following morning he received this note from his patient:—"It has just occurred to me that I may have hydrophobia, as I can't touch water. I was bitten by a mad pup about two and a half years ago at Agra in the right hand. My right arm pained me a few days ago for no apparent reason." The doctor found the poor man "suffering from all the symptoms of well marked hydrophobia."

The details of the case are too long for insertion. The patient suffered terribly, and died from exhaustion on the 28th, at about 5 P.M.

The supposition that the case might be one of nervous terror simulating hydrophobia is met by the fact that the patient thought the pain in the limb to be rheumatism, and "it was not till the early morning of the 28th—the day on which he died—that the awful reality flashed through his mind, when he found that he could not swallow fluids." Dr. Freyer had seen cases of hydrophobia before, and the medical officers who saw the case pronounced it hydrophobia.

As to treatment; hypodermic injections of morphia (to one of which was added  $\frac{1}{4}$ th grain of atropine) gave some relief.

Attention is called to the long period of incubation ( $1\frac{1}{2}$ , not  $2\frac{1}{2}$  years as patient stated). "The wounds were not cauterised at the time as the pup was not supposed to be mad." (It subsequently died mad.) The writer strongly urges the necessity for excising or cauterising bites from animals. Even although there is no evidence at the time that the creature is mad, cauterisation should be practised. If these steps are neglected at the time, the cicatrix—(with a margin)—should be excised.

"Pasteurism"—By a Subject.—This is an interesting letter from "an Indian Medical Officer, who was unfortunately compelled by the bite of a rabid dog to proceed to Paris, and place himself under Pasteur's treatment." The writer is an enthusiastic believer in the value of Pasteur's treatment. (In the May number of the *Gazette* a correspondent gives the other side of the question).

**Contagium of Scarlet Fever.**—Some interesting researches have lately been conducted in Edinburgh by Dr. Edington—the object being to discover the "germ" of scarlet fever. These researches were undertaken in connection with Dr. Jamieson's prophylactic treatment. Dr. Edington found a particular bacillus in the blood, early in the disease, and in the desquamating epithelium, at a later date. This organism was found capable of giving rise in animals to a disease resembling scarlet fever.

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ORIGINAL ARTICLES.

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THREE CASES OF BRAIN AND EAR DISEASE, CON-  
SIDERED WITH REFERENCE TO DIAGNOSIS, AND  
ALSO TO QUESTIONS OF BRAIN SURGERY.

By W. T. GAIRDNER, M.D., LL.D.,  
Professor of Medicine in the University of Glasgow.

THE importance of the following cases to the clinical observer and to the surgeon depends on the coincidence of certain cerebral symptoms with evidence, obtained during life, of a very old and disorganising lesion of the middle ear. All such cases, under the views and practices recently advocated, may be said to involve questions as to the possibility of relief through surgical interference; and even when these questions are not fully entertained, or, as in Case I, are carefully considered and decided in a negative sense, the narrative cannot fail to be of interest. These cases may be read, indeed, as a contribution to the important discussion in the Medico-Chirurgical Society of Glasgow in February last, the more so as Dr. Gairdner was obliged to admit, in submitting Case I to the Pathological and Clinical Society, that in the course of last winter, at a time closely coinciding with the successful case of Dr. Gowers and Dr. Barker, a case had occurred in his wards (Case II) in which, as the *post-mortem* examination showed, a successful result might have been obtained had the observations of the facts during life appeared to justify the surgical procedure adopted in Dr. Gowers' case.



CASE I.—*Acute or sub-acute disease in chest, with probably incipient delirium tremens, the result of excesses; apparent convalescence; sudden rise of temperature, with progressive coma; fetid discharge from ear; hemiplegia on the opposite side to the lesion of the ear, and temporary cataleptic rigidity of the non-paralysed side; hyperpyrexia; death; post-mortem examination; basilar meningitis, &c., &c.; no tubercles, and no lesion of brain substance; ear carefully examined during life and after death by Dr. Barr.*

H. T., æt. 24, a seaman, was admitted to the Western Infirmary from the Seaman's Home, on 8th March, 1887. His symptoms on admission were not distinctively cerebral, but rather thoracic, except in so far that it was ascertained that he had been, during three months' residence in Glasgow, indulging in every kind of dissipation to such excess as to be suspected, even before his admission, of *delirium tremens* in a mild form. He complained of headache, and said he felt "strange" on the day after his admission; but the most careful scrutiny of the facts showed nothing that was not fairly in accordance with the physical signs actually observed—viz., condensation of the left lower pulmonary lobe, with suspicion of pleurisy and also of pericarditis; traces being found also of herpes on the lips and of pneumonic expectoration. After a somewhat delirious night, he became so far convalescent and rational as to be allowed, at his own instance, to give considerable assistance in the ward; and although the evening temperatures still showed more or less of fever, there was no reason to apprehend anything more serious until the 17th, when the *morning* temperature suddenly rose to 105·2°, and maintained a high level throughout the day—this alteration in the temperatures coinciding with a rapidly developed, though not extreme, comatose condition, without any muttering or any active delirium; the pupils inclining to dilatation, and very little influenced by light. At this date also the foetid discharge from the left ear was first observed, of which it may be sufficient to remark that subsequent inquiries among his relatives showed that it had probably its origin in scarlet fever at ten years of age; but, although known to have existed at that time, it had probably been either intermittent or entirely neglected in the interval, as no information could be obtained about it till the staining was observed upon the pillow. Dr. Barr was asked to examine the ear at this date, and found foetid purulent secretion in the canal, on removing which the tympanic membrane was seen to be entirely

destroyed, with granulation tissue, evidently due to an old inflammatory process. On the 18th, the only difference observed was a slight (almost doubtful) trace of paresis of the right side of the mouth, which had the effect, however, of leading to a very minute investigation of all the other signs of cerebral or spinal paralysis, with negative result. *Tache cérébrale*, however, of a singularly persistent kind was detected at this date, and from this time onwards the evidences of structural changes within the cranium may be said to have been always present and more and more pronounced. On the 19th, there were still some slight signs of remaining consciousness, as in putting out the tongue slowly in response to a verbal request. On the other hand, hemiplegia, as regards the right upper and lower extremity, were very apparent for the first time; and, what was especially noted as a very curious peculiarity, a partially cataleptic rigidity of the non-paralysed (left) limbs existed, affording a singular contrast with the relaxed and motionless condition, especially of the right arm, even under reflex stimulation. These facts were witnessed by Dr. Finlayson in consultation with Dr. Gairdner, and it was also observed that the slight paresis of the portio dura, as regards the right side of the mouth, was not accompanied by the usual phenomena indicating lesion in the trunk of that nerve, nor yet by any well characterised ocular paralysis; the tongue also appeared to be protruded in the middle line. Dr. Reid examined the eyes at this date, and found a low degree of optic neuritis on the left side only, with considerable congestion and tortuosity of the retinal vessels on both sides, especially on the left. The temperatures, pulse, and respiration were all less abnormal than in the early stage of the coma, but showed no other peculiarity, except that Cheyne-Stokes respiration was distinctly observed, not constantly, but on several occasions during the 17th-19th March. (This symptom did not increase, at least until the patient was moribund.) It may be noted as particularly bearing on the question of abscess, that no rigors were at any time observed. On the 21st, the patient being in nearly the same condition, a special consultation was held, at which Dr. McLeod, Dr. Macewen, Dr. Barr, and the medical officers of the ward were present, with the view of determining whether any surgical proceeding should be adopted, on the hypothesis of a possible abscess, associated with the destruction of parts in the middle and internal ear. It ought, perhaps, to be added that the facts submitted to judgment at this consultation were very

much more fully recorded than appears from this brief abstract, and were carefully gone over by the surgeons individually in their connection with the history and the symptoms then present. In particular, some clinical observations of special interest connected with the heart, which, from a desire for brevity, are entirely excluded from the present communication, may have had their share in contributing to the impression created of a very complex diagnostic position. The result of the whole was a concurrence of opinion that the facts and inferences were too uncertain, and the state of organic change too complicated, to permit of a clear decision such as would justify an operation. This opinion was stated to the relatives and friends of the patient, who, occupying a station in life considerably superior to his, had come from long distances on hearing of his perilous condition. Soon after this the coma became more profound, the temperatures rising again to  $105^{\circ}$ , and, a few hours before death, to  $106.2^{\circ}$ ; the pulse and respiration also becoming greatly disturbed and very rapid. Death took place on the 22nd March, exactly a fortnight after admission, and three weeks after the ostensible beginning of the illness as a sub-acute chest affection, complicated, apparently, by *delirium tremens*, as was ascertained both from Dr. Hardy, who attended him in the Sailors' Home, and from the house manager, who gave distinct evidence as to his habits for some months previously. The chest symptoms were entirely in abeyance after the cerebral condition became pronounced, but the physical signs observed still persisted.

*Post-Mortem Examination* on 23rd March (Dr. Coats).—The body is very well nourished. On exposing the brain by removing the dura mater, the convexity is seen to present at intervals a yellow exudation occupying the sulci, equally on right and left sides. At the base there is an abundant yellow exudation beneath the arachnoid, extending from the optic commissure into both Sylvian fissures, and over the pons and under surface of the cerebellum to within a quarter of an inch from the posterior border. It is also present abundantly on the upper side of the cerebellum. Lateral ventricles somewhat dilated; the fluid is turbid, with a trace of yellow matter in the most dependent part. The velum interpositum, especially in its central parts, is somewhat infiltrated with yellow exudation. There are a few drops of pus in the 4th ventricle. Membranes of the brain generally are somewhat injected, and *puncta hæmorrhagica* unusually abundant. No lesion is found in the brain substance. The sinuses of the

dura mater, and the *dura mater* itself, present nothing abnormal. The frontal sinuses are found to be occupied by a somewhat tenacious yellow matter. (No tubercles were discovered in the brain or membranes.)

The other appearances were—a lesion of the aortic valves, which, with a hæmorrhagic exudation in the substance of the spleen (which weighed 16 oz.) and a bulky thrombus in the main splenic artery, gave some considerable support to the theory of embolism as the source of the cerebral affection, although the facts above stated failed to demonstrate this. It will also be noted that although the clinical facts pointed very distinctively to a unilateral lesion of the motor centres in the brain, no such lesion was actually discovered. Nor can it be said that the connection of the basal meningitis with the disorganising changes in the middle ear was distinctly made out.

Dr. Barr examined the left temporal bone, and has furnished the following notes:—

Careful examination of the osseous partitions between the cavities of the ear and the intra-cranial cavity failed to discover any caries or necrosis. The venous sinuses, in relation to these partitions—namely, the lateral, the superior petrosal, the inferior petrosal and the cavernous, as well as the internal carotid artery and the bulb of the internal jugular vein, were slit open without showing any morbid condition. The dura mater covering the *pars petrosa* seemed to be in no way affected. On removing the antero-inferior wall of the external meatus, the *membrana tympani* was in great part destroyed, only a small portion in front and behind the short process of the malleus, and a narrow, tendinous looking rim at the periphery remained. The lower third of the manubrium was absent, probably from carious erosion. Close to the postero-superior periphery of the membrane, the lenticular process attached to the head of the stapes was seen, but the long process of the incus was represented by only a thin membrane. The tendon of the stapedius muscle remained in proper position. The inner wall of the tympanum, white in colour and lined still by periosteum, was exposed in all its detail, while the tessellated structure forming the floor of the tympanum was likewise visible, but bereft of periosteum.

The roof of the middle ear was removed, and through the gap thus made a very instructive view was obtained. In the mastoid portion was a deep pyramidal cavity, about an inch in depth and three-quarters of an inch in breadth. This cavity, which was evidently a greatly dilated *antrum*, was

filled with thin sero-purulent secretion, and its inner wall was separated from the groove for the lateral sinus by a very thin, indeed translucent lamina of bone. It communicated with the tympanum at its upper part by a comparatively small aperture. Looking down into the tympanic cavity were seen the head and neck of the malleus, with part of its manubrium, also a small remnant of the incus and the two crura of the stapes. The tendons of the stapedius and tensor tympani were also visible in their normal situations. In order to see these contents of the tympanum, a pseudo-membranous structure, which was enveloping them, had to be removed.

The condition of this middle ear was typical of the ravages which are frequently produced by an old standing purulent disease when originating in scarlet fever.

In the following case, it must be quite frankly admitted, in the light of the *post-mortem* examination, that an opportunity was missed of performing what might very well have been a successful operation for the relief of cerebral abscess. But the circumstances under which it was presented were not favourable for entertaining the question, even had the successful issue of Dr. Gowers' case (which had occurred very shortly before) been duly considered. There were no localising symptoms, other than the fact of the ear disease, almost no fever, and no rigors. The patient was seen by Dr. Gairdner less than 40 hours before death, and although it was undoubtedly probable that the cerebral disease was dependent on the lesion in the ear, the nature of the association, and the character of the intra-cranial disease, were considered doubtful until fully revealed by the *post-mortem* examination.

CASE II.—*Deafness and discharge from left ear, probably for five years; otherwise good health; three weeks before death acute pain in the ear, extending to the head; five days before death delirium and lethargic stupor, but no rigors, convulsions, nor paralysis; death by coma, with notable pyrexia only for a few hours; abscess of brain distinctly traced to the disease of ear.* (Dr Barr.)

John Q., æt. 54, barber, was admitted into Ward I, Western Infirmary, under Professor Gairdner, on 15th December, 1886. A very careful record was made immediately on his admission, of the facts then available, but as this was necessarily imperfect, owing to his not being in a condition to give any information, it may be stated here that it was ultimately ascertained from his wife, and

otherwise, that he had been a perfectly healthy man, with insignificant exceptions, and that no hereditary disease of any kind could be traced. His only infirmity had been deafness, and there were discrepancies in the statements as to the existence or not of a discharge from the left ear previous to the present illness; but he had consulted several advisers, for about five years, on account of the deafness. The last occasion on which he so took medical advice was the only one on which even any suspicion of cerebral disorder could be ascertained.

About three weeks before admission he seems to have suffered considerably from a pain in the left ear, "so acute as to make him nearly distracted," but in the first instance, at least, confined to the ear. On the 4th December (15 days before admission), a cartilaginous growth was removed from the meatus of the left ear, and on the 5th a tooth was drawn, both of these operations being performed with a view to the relief of the pain, and, apparently, without any difficulty, but also without any sense of relief. The pain, so far from being removed, spread to the occiput and afterwards to the whole head, blisters being applied on account of it on the 11th and again on the 17th December. It may be taken as fairly well established by evidence that no important change as regards his intelligence occurred up to the 16th (three days before admission), and that there was neither vomiting, convulsion, nor paralysis observed at any time up to admission. Three days before admission he was observed to have "a relapse," attended by some degree of wandering in his talk; and from this time onwards a lethargic stupor, deepening into coma, seems to have been the only indication of progressive cerebral disease. Fever was inconsiderable, and even after admission, the temperature on two occasions (20th) was noticed as absolutely normal; but on the 21st a sudden rise took place to  $105.2^{\circ}$  at 1.20 A.M., and from this time onwards till death at 4.35 A.M., high temperatures were maintained. Unconsciousness (with stertorous breathing, as in apoplexy) was complete for five or six hours before death; but although during the period of observation (less than 40 hours) every possible attention was given to the details, no facts tending to localise the lesion were observed; no paralysis of any kind nor abnormal movements of the limbs, nor of speech, although at the first examination he was able, in a kind of somnolent, lethargic fashion, to walk about when desired to do so, and even to utter a few words in response to questions. His

wife stated that he had been feverish in the week before admission, but it did not appear that any rigors were observed. It may be noted that the *tache cérébrale* was undoubtedly present (though in a much less marked form than in the preceding case); the respiration was calm and natural up to within a few hours of death. The evacuations were normal up to within three days before death, when retention of urine took place, requiring relief by the catheter. The pulse, on admission, numbered about 58, and had no peculiarity of character. Death occurred on the 21st December.

*Post-Mortem Examination* on 22nd December (Dr. Coats).—On exposing the brain, the left hemisphere is seen to bulge considerably as compared with the right. The veins generally are hyperæmic. The inferior surface of the left temporo-sphenoidal lobe is adherent over a small area to the dura mater; and in removing the brain this adhesion is torn through, and simultaneously an abscess in the brain is laid open, from which escapes a quantity of thick, flocculent, and very foetid pus. At the place of adhesion this abscess is close to the surface, so that there is only a thin layer of white matter overlaid with red, between the cavity and the surface. The neighbouring brain substance is much undermined, and has a dark bluish colour. The abscess measures about  $1\frac{1}{2}$  ins. from before backwards, and  $1\frac{1}{2}$  ins. transversely; it is lined with a distinct yellow layer, whose internal surface is irregular and flocculent. The abscess is situate in the left temporo-sphenoidal lobe, the middle point of it corresponding with the place of the adhesion, being  $2\frac{1}{2}$  ins. from the anterior extremity of the lobe. The superior temporo-sphenoidal convolution is not engaged, but the middle and inferior convolutions are largely undermined. There is pus in the sub-arachnoid space, over the inferior surface of the cerebellum, and over the optic chiasma, extending also slightly into the fissure of Sylvius on both sides, but especially on the right. It is everywhere inconsiderable in amount.

The dura mater presents a perforation, with shreddy irregular margin, opposite the point of adhesion to the brain. The bone beneath is irregular, and covered with a semi-purulent flocculent matter which extends over a considerable area, nearly as large as the upper surface of the petrous bone. At the internal margin of this area there is a perforation of the bone, just in front of the superior semi-circular canal; and a probe introduced here passes freely out at the external meatus.

Dr. Barr examined the temporal bone, and found on removing the cartilaginous part of the external auditory canal and the tympanic plate of the osseous portion, that a fibrous polypus, half an inch in length, existed in the inner end of the canal, springing from the posterior wall. A carious opening communicating with the antrum mastoideum also existed in the same position. Two carious apertures were also seen on the upper surface of the *pars petrosa*, one through the roof of the tympanum, and the other through the roof of the antrum. The *membrana tympani* was in great part destroyed, only a small portion at the antero-inferior periphery remaining. In order to avoid disturbing the carious openings, no further sections of the ear were made.

CASE III.—*Diabetes mellitus and albuminuria, of indefinite duration; absence of knee-jerk; no other obvious symptom of disease of innervation till within two months before death; thereafter, paralysis of left portio dura, attributed to, and probably dependent upon, disease of left middle ear, recognised only after admission; no other paralytic symptom till within 24 hours before death; when, after very copious epistaxis, progressive coma, with hemiplegia of the entire right side, were observed; temperatures (previously not exceeding 100.8°), rose after the epistaxis to 103°, then to 104°, continuing elevated till death. Post-Mortem—disease of left petrous bone; thrombosis of left internal carotid artery, with secondary changes in the convolutions of left brain; no corresponding changes on right side; clinical and pathological details and remarks.*

Mrs. Isabella W., æt. 54. Admitted in the beginning of February, 1887, to Professor Sir George M'Leod's Wards, from which she was transferred to Dr. Gairdner's Ward, No. VI (Journal DD, p. 133), on 21st February, remaining there till her death on 4th March. The following abbreviated account of the symptoms and history will include facts recorded by several observers, reserving Dr. Barr's more detailed observations on the ear for a separate statement in his own words. The case was very carefully considered by Sir George M'Leod, in respect of its having been sent to him as possibly one fit for a surgical operation. The complete paralysis of the left portio dura in all its external branches, and the disease of the middle ear on the same side, were at this time the leading symptoms. There was



a history of cephalalgia, at first supposed to be neuralgic, and confined to the left side of the head, but afterwards extending "all over." The other local symptoms were unimportant; but there was evidence of some possibly trophic disorders of innervation (cold feet, numbness, spontaneous blistering of feet, achromatopsia, &c.), of which, however, no very accurate or connected narrative was obtained, such as to show a distinct cerebral lesion. Her mental state at this time (with the exception of some apparent lapses of memory) seemed unexceptionable. There was no paralysis, except of the left portio dura; and this, while extending over all the external branches, was particularly observed not to involve the geniculate ganglion, the action of the muscles of the soft palate being normal. The knee-jerk was wholly wanting in both limbs; but no other characteristic feature of locomotor ataxy, or of any spinal affection, was present. The details of observation corresponding with the above statements occupy a considerable space in the report, but may, for convenience, be omitted here. There was a considerable failure in her eyesight, but it remained doubtful, after an ophthalmoscopic examination, how far this was connected with the other symptoms, as above described. The urine had exceeded 100 oz. in the twenty-four hours, while under observation in the surgical ward, and in the medical ward, 62 to 89 oz.; it contained 29 to 33·6 grs. of glucose per ounce; and had a sp. gr. from 1035 to 1040. Albumen copious. Urea, 1·8 to 2·2 per cent. It was found absolutely impossible to determine, even approximately, the period of commencement of these symptoms.

On 3rd March (the patient having been much in the same condition as above described since the middle of January, but always with a good deal of pain in the head) copious epistaxis occurred in the afternoon, leading to the loss of about 36 oz. of blood, when it spontaneously ceased about 5 P.M. The temperature at this time was found to be 103° (having been scarcely at all abnormal for several days before), and from this moment onwards a lethargic condition existed, verging towards coma, and associated with hemiplegia of the right limbs; but not, so far as observed, any sudden or apoplectic seizure, such as usually follows a hæmorrhage, or even an embolism. On the 4th March, at the ordinary morning visit, "the patient lies drowsily, and apparently automatically moving her left hand and arm only, occasionally responding feebly by gestures and move-

ments of the lips to words spoken, but in no instance rousing up so far as to give an answer to a question verbally, and not seeming to recognise friends who had come to see her. The testing of sensibility under these circumstances gives necessarily ambiguous results, but on the whole it would seem that the reflexes in response to pricking, pinching, &c., are more active on the left than on the right side. The plantar reflex is extremely sluggish in both limbs, the movements of the eyes do not appear to be affected, nor are the pupils at all unequal." In this condition, the patient continued up to 8 P.M., when she was perfectly comatose, with a temperature of  $104^{\circ}$ , respirations 30 to 36 per minute; pulse rapid and irregular. She died at 11 P.M., about sixteen hours after the cessation of the epistaxis which seemed to mark the first notable acute change in her cerebral condition. Clinically considered, therefore, the disease may be said to divide itself into three stages, characterised by—1st, Diabetes, glycosuria, and albuminuria (of indefinite duration); 2nd, Disease of middle ear, perhaps extending out of the previous stage, but afterwards associated with discharge and with paralysis of the left portio dura in its passage through the bony canal (two months at most); 3rd, Acute cerebral disease on left side, giving rise to right hemiplegia and coma within twenty-four hours before death.

*Post-Mortem Examination* on 7th March (Dr. Coats). See Glasgow Western Infirmary Pathological Reports, No. 1,683.

The surface is very pale; the body moderately well nourished. There is a vesicated surface on the right heel (spontaneous vesication above referred to).

The heart is normal in size; the septum, as viewed from the left ventricle, shows a patch of fibrous tissue, and is here very thin. The heart otherwise is normal; weight,  $10\frac{1}{2}$  oz. The lungs are not adherent; there is considerable œdema in the posterior parts. Liver has a somewhat mottled appearance, and weighs 4 lbs. 3 oz. Spleen somewhat enlarged and soft; weighs 6 oz. Kidneys  $5\frac{1}{4}$  and  $5\frac{1}{2}$  oz.; nothing remarkable to the eye; under the microscope, cloudy swelling with slight degeneration of the epithelium. There are tube casts, hyaline and granular. There is no acetone reaction in the urine as taken from the bladder. The abdominal organs are surrounded by thick layers of adipose tissue.

*Head.*—The calvarium is very heavy and the bone thick, especially at the frontal region, where it is  $\frac{3}{8}$ ths of an inch. On removing the dura mater, a large excess of fluid is observed beneath the arachnoid, filling out the sulci; the convolutions

present a corresponding atrophy; the cerebral ventricles are slightly but not excessively dilated; and the brain substance, as a whole, is remarkably firm in texture. On removing the soft membranes there are observed, on the left side, on the surface of the convolutions, somewhat frequent areas of a pinkish colour, and usually about  $\frac{1}{4}$  inch in diameter. It is also found that on this (left) side the membranes are occasionally adherent to the surface of the brain, the grey matter of the convolutions presenting on section a distinct pinkish colour. The convolutions on the right side present no lesions further than the atrophy already mentioned. The white substance of the brain presents not unfrequently an appearance suggestive of that described by Dickinson (in diabetes). The anterior face of the petrous bone on the left side presents a perforation, and pus is seen at this point, but not extending to the dura mater. A probe passed through this opening readily issues at the external meatus. There is softening extending from the point of perforation inwards, and involving the apex of the temporal bone, where there is a considerable cavity. This cavity communicates with an erosion of the bone posterior to it, and close to the foramen magnum. The parts around are considerably matted, and the carotid artery is adherent in its canal.

On further examination of the petrous bone, especially by cutting up the internal carotid artery, it is found that the latter is the seat of a firm white thrombus. The thrombus is adherent in the perpendicular portion of the artery, and its tapered end extends into the transverse part, there being a longer tail-shaped piece attached to the apex. [Further details as to the pathological anatomy of the ear will be found in Dr. Barr's remarks, *infra*.]

*Microscopic Examination* of the brain in the fresh state shows, in the red areas above described, a very striking fatty degeneration of the vessels, such as occurs in areas of cerebral softening, as from embolism. Stained sections show the vessels surrounded by round cells, each vessel being in a state of inflammation.

At this point the actual notes made for the purposes of Dr. Gairdner's clinical lecture, on 8th March, may here be inserted, as they contain, in a very condensed form, the whole argument as to the diagnosis, freshly gathered from the clinical observations, and carefully considered with a view to the instruction of students. It is necessary, however, to remark that, although the first observed and most obvious facts of the *post-mortem* examination, performed on the 7th March, formed

a part of the materials of the lecture, the rare and most interesting observation as to the thrombus in the carotid, acting as the intermediary between ear and brain disease, remained undiscovered till later, when the parts were subjected to further examination both by Dr. Coats and Dr. Barr. The clinical facts, even apart from this, however, appeared sufficiently rare and curious to demand a more exhaustive treatment than was possible in a lecture prepared at the time, and the class was therefore led to look to the ultimate publication of the case as one of unique value and interest.

*Remarks on Case of Mrs. W.*, inscribed in the Journal of Ward VI, DD, p. 175, under date 7th March, 1887, by Professor Gairdner, with a view to clinical lecture.

1. The association of paralysis of the portio dura with glycosuria appears to be so rare that no inference can be based upon collection of previously recorded facts of this nature (indeed, up to this date no such case has been discovered). Nor can the literature of medicine, so far as consulted, be said to throw much or any light on the hemiplegic incidents of this case in their connection with glycosuria on the one hand, or albuminuria on the other.

2. The comatose attack in this case might (apart from the hemiplegia) have passed for a form of "diabetic coma," perhaps, by preference, the second of Dreschfeld's three types; but the absence of acetonæmia, and the presence of cerebral lesions with hemiplegia, remove it from that category.

3. The fact of the portio dura paralysis being universal as regards the external branches, but leaving the velum pendulum and uvula undisturbed, led to the inference (during life) that the lesion *quæ* the portio dura was *not* cerebral, but in the tract of the nerve itself, and more specifically, *outside* the geniculate ganglion. The portio mollis, too, was probably affected, chiefly through the mechanism of the inner and middle ear, and not at its nucleus, or in its course to the foramen.

4. The only *single* cerebral lesion that could, under any circumstances, have explained the fact of paralysis of the face on left side, concurring with right hemiplegia (crossed or alternate paralysis) would have been a lesion in the pons, or, more specifically "a lesion in the lower or posterior part of one lateral half of the pons, because in this place a single lesion may implicate the outgoing fibres of the facial (that is, its root fibres) after their decussations in the pons, and also the channels for the conveyance of motor stimuli to the limbs

before their decussation in the medulla.”\* But a lesion so situate as to implicate the nucleus of the facial nerve in the pons would also probably implicate the sixth, which “arises from a nucleus common to it and the portio dura.” (*Loc. cit.*) Hence the importance of the observation made in this case as to the absence of any lesion of function in the ocular muscles; and also the absence of any symptom of what is now known as “bulbar paralysis.”

5. The inference, therefore, from all the facts up to the time of the hemiplegic seizure was that the paralysis of the portio dura was peripheral (not cerebral), and that the lesions of the brain, if any, were only remotely—*i. e.*, not essentially, connected with the facial paralysis. The only ground at this time for suspecting a cerebral lesion at all was the persistent headache, more general than could be easily attributed to an ear origin. The instability in walking, and the “giddiness” (noticed on one occasion in the *Journal*, p. 136) may have been of labyrinthine origin, or of the nature of “Menière’s vertigo.” The early trophic incidents and the loss of the knee-jerk might (under certain circumstances) have portended a centric or spinal lesion, but the latter, at least, may have been due to the diabetes.

6. The hemiplegia and the death by coma (the latter not being acetonæmic) portended with great probability a cerebral lesion or lesions on the left side of the brain, but distinct from, though possibly arising out of, the lesion which produced the portio dura paralysis (in the pars petrosa). This cerebral (and late) lesion *might* have been an abscess, or more than one; but there was no evidence as to their exact localisation, if present. (Hence no room for brain surgery).

7. The albuminuria, with tube-casts, might have raised a question of uræmic coma; but this is not usually, perhaps not ever *per se*, a cause of hemiplegia. The state of the kidney after death, accordingly, shows that the albuminuria was probably a secondary and incidental fact: at all events, one not of long standing, nor attended with much renal degeneration.

8. The serous exudation within the arachnoid and pia mater was probably determined by the atrophy of the cerebral substance; perhaps, also, in part by the epistaxis (as in animals bled to death). “Serous apoplexy” is probably a misnomer, in so far as it assumes as a cause what is more probably an effect. Moreover, merely serous exudation will not, *per se*, account for hemiplegia.

\* Bastian, *Paralysis from Brain Disease*, pp. 146-50, where this subject is ably and exhaustively discussed.

The following remarks, kindly furnished by Dr. Barr, may conclude the narrative of this exceedingly rare, if not unexampled case of associated brain and ear disease:—

Case of death from hemiplegia of right side associated with facial paralysis of left side, in which the order of events was the following:—(1.) Inflammation of left middle ear. (2.) Extension of this inflammation to Fallopian canal, causing facial paralysis. (3.) Extension of inflammation in middle ear to internal carotid artery in carotid canal, causing thrombosis in artery. (4.) Formation of minute embolic lesions in brain. (5.) Hemiplegia of right side. (6.) Death.

*Examination of temporal bone by Dr. Barr.*—On removing the cartilaginous part of the external auditory canal and the tympanic plate, two or three small soft blood clots were seen to occupy the inner part of the canal. The tympanic membrane was red and moist, with bloody serum on its surface. An elliptical gap about 3 mm. in length and 1 mm. in breadth existed in the antero-inferior part of the membrane, through which a probe passed when pushed into the osseous part of the Eustachian tube. Through this gap the mucous membrane of the tympanic cavity was seen to be red and tumefied; and some reddish, serous fluid was mopped up with cotton from its interior. Owing to the softened state of the membrane and the necessary manipulations the gap became enlarged in length and breadth, and now it includes fully a fourth part of the membrane.

I removed very carefully the osseous roof of the middle ear over Eustachian tube, tympanum, and antrum. The antrum contained thin serous fluid, which seemed, indeed, to permeate the osseous tissue forming the roof of the middle ear. The tympanic cavity was full of a soft, red, granular-like tissue, filling it like a padding and apparently consisting of proliferated mucous membrane. The ossicles were embedded in this material, so that they could not be cleared of it without endangering their integrity or dislocating them. In looking down into the tympanic cavity the head of the malleus and the body of the incus were seen peeping up from amid this tissue. The mucous membrane lining the tympanic opening of the Eustachian tube was swollen, and had a dull red colour. Dr. Coats had previously opened into the internal carotid artery, as it emerges from the upper end of the carotid canal in the petrous part and found a *præ-mortem* thrombus, the thickest portion of which was at the angle where the canal bends from the vertical to the horizontal. The inner lining

of the artery was distinctly red as compared with the adjoining internal jugular vein.

I did not find any carious or other aperture over the tympanic cavity, but through the osseous part of the Eustachian tube, just about its junction with the cartilaginous part at the outer side of the carotid canal, a probe could be pushed into the Eustachian tube, and then through the aperture in the membrane. The osseous part of the Eustachian tube, just at its junction with the cartilaginous part, is generally very ragged and irregular, the union being sometimes incomplete, and there is no doubt that at this part a probe might, in some cases, after the dura mater is removed, be pushed into the tube. The existence of the drop of pus is difficult to explain in view of the fact that no purulent secretion existed in the middle ear or external auditory canal.

In regard to the aperture in the tympanic membrane, I think it is possible that this may have been made by the probe when first introduced through the Eustachian tube. It existed precisely at the place where a probe passed through the tube would come in contact with the membrane. In the extremely softened state of the latter, very slight force would suffice to penetrate it. No such gap existed in the membrane when I examined the ear, about two weeks before death.

*Observations by Dr. Barr.*—When I saw the patient in Professor Macleod's ward, the symptoms were those of acute catarrh of the middle ear on the left side, complicated with circumscribed inflammation in the external auditory canal, causing some discharge from the ear. There was no history, beyond that of a few days, of discharge from the ear. The hearing by air-conduction was bad: a watch, which is heard in normal circumstances 40 inches from the ear, was not heard on pressure; hearing by bone-conduction was, however, good, indicating disease of the conducting structures only. After cleansing and drying the canal of the ear, the tympanic membrane was seen to be congested, but no perforation could be detected either by inspection or by inflation and auscultation. Inflation seemed to slightly improve the hearing power, and I suggested its repetition at certain intervals. The facial paralysis was probably due to simultaneous inflammation of the neurilemma of the nerve as it lies in the contiguous Fallopian canal. It is a mistake to think that facial paralysis of aural origin is always due to a *purulent* disease of the ear. I have seen several cases of facial paralysis resulting

from simple non-suppurative catarrh of the ear, the paralysis passing off with the catarrhal process. Von Tröltzsch, Wilde, Dalby, and others report similar cases. This would be expected from the intimate relation of the Fallopian canal to the mucous lining of the tympanic cavity. On the inner wall of the tympanum, just behind the recess of the fenestra ovalis, the neurilemma of the nerve is separated from the mucous membrane by only a very thin osseous partition, which is sometimes defective, containing little gaps where the mucous membrane and the nerve are in contact. What is more likely than that congestive processes in the one should also affect the other? Besides, the vascular supply of the two parts is intimately connected by the stylo-mastoid artery which, passing up the facial canal, supplies the nerve and a considerable part of the tympanic cavity. It is, I think, extremely probable that the thrombus in the carotid was the consequence of the inflammation of the middle ear. In the anterior part of the inner wall of the tympanum, only a thin porous partition separates the mucous membrane from the walls of the artery, while two or three twigs are at this place given off by the artery to supply the tympanum. The inflammation has no doubt passed into the adjoining walls of the artery, either along the small arterial twigs or through the apertures for connective tissue. At the point where the artery bends at almost a right angle, a coagulum would be specially liable to form under the influence of inflamed walls. The danger of the formation of a thrombus in the carotid artery at this part, in connection with inflammation of the middle ear, is mentioned by many writers, although extension to the neighbouring venous channels is, of course, regarded as a much more likely event; but I have failed, in going over the literature of the subject, to find any recorded case. Many instances are known of erosion of the arterial walls from purulent inflammation of the middle ear leading to fatal hæmorrhage, but, so far as I know, there is no recorded case similar to the one now before us.



ILLUSTRATIONS OF SYPHILITIC DISEASE IN THE  
NERVOUS SYSTEM.

By JOHN AIKMAN, M.D.

BECAUSE these cases illustrate what are undoubtedly truths about syphilis, I have thought them worthy of record. The cases are typical rather than unique or even very rare, and in this lies much of their value. They demonstrate, what is too often lost sight of, that syphilitic nerve lesions more often follow slight secondary manifestations than an infection which has appeared largely in the secondary stage. They also demonstrate that coarse lesions of the nervous structure are more readily affected by anti-syphilitic treatment than the finer lesions. These are facts, whatever may be the explanation of them. We may assume, as Dr. Mortimer Granville does, that the virus travels along the nerve fibres to the proximate centre, and thence, in course of time, affects the whole nervous system. We may believe that the bodily vigour of the patients combats the disease until it acquires the age or attenuation which renders it specially liable to attack the nervous structures. These theories do not affect the fact that syphilis is not eliminated because its secondary stage is suppressed by any other cause than active anti-syphilitic treatment. The most natural means of cure is a vigorous constitution, a careful life, and perhaps some points in inheritance. A patient who is in such way resisting syphilis without the aid of drugs may improve in health and gain in weight, and yet retain the syphilitic infection. But he will still further improve in health and spirits and gain in weight under full doses of iodide of potassium. No person who is free from syphilitic taint will continue to gain in health and weight under similar treatment. Herein is some criterion of the elimination of syphilis. But it is not true that nerve lesions are the only symptoms of long resisted syphilis, though they are apt to be the most obviously syphilitic. I have at hand an instance in which chronic catarrh of the nasal mucous membrane troubled a patient for three years without ulceration or any necrosis of the nasal bones. No treatment availed, and the patient denied any possibility of syphilis. An eminent specialist thought the disease non-specific. But some suspicious ulcers showed themselves on the hard palate, and on pressing the history to its fullest there was an admission of warts on the penis seventeen years before. These, treated

with chromic acid, led to a suppurating bubo in the groin. There was no other sign, but the first indication of trouble in the nose dated from a severe attack of rheumatic fever followed by much debility. A tentative trial of iodide of potassium in this patient, who had a splendid physique, not only cured his nasal catarrh and palatal ulcers, but increased his bodily vigour to a standard which he had not known since his attack of rheumatic fever. In this instance the bodily vigour could resist the ulceration of mucous membrane and necrosis of turbinated bone, which would have been markedly syphilitic, but could not resist an imperfect manifestation in a common seat of selection. Would the symptoms have been more characteristically developed had they occurred in the nervous system? If so, there is some ground for the belief that the primary infection of the nervous system is peculiar. There is a manifest parallel between such belief and the statement already made that coarse lesions of the nervous system are more amenable to treatment than the finer lesions. For if we inquire into the meaning of coarse lesions, we find that they mean signs of external pressure from growths—mostly inflammatory and specific—of the fibrous structures, while fine lesions are more truly nervous. And so it is that this instance of not nerve syphilis throws some light upon cases which, from their symptomatology, we must at first sight class as nerve syphilis.

There must arise later a question as to the causes which determine the syphilitic virus to the nervous system. Suffice it for the present to say that there is not sufficient reason to believe that it concerns the virus itself, despite Mr. Hutchinson's records. It requires some gross nervous lesion such as is portrayed in the following case to realise that the nervous system may be deeply involved by syphilis and yet not in syphilis. So perfect a structure as the nervous system never would, and never could, have recovered with the completeness which this case records, had its nerve tubes actively taken up the morbid process.

CASE I.—A woman, aged 40, badly housed, badly nourished, badly cared for, and in previous life intemperate. Was first seen in bed, semi-unconscious and paralysed. For fifteen weeks she had great pain in head—first over nose, then on vertex, and lately more difficult to define. The pain was dull in character: she moaned rather than called out. She was supposed to have "a tumour in her brain." The mental faculties were lowered and bewildered. She had kept her

bed for five weeks, was under the belief that she was far from home, and before she was bedridden would get out of bed at night on all sorts of errands, mostly to make food or drink.

She had now a normal temperature, a pulse of 58, and a respiration rate of 17-18.

Her expression was vacant and somnolent, but she was markedly emotional when roused. Her facial lines were mostly effaced on the left side, and there was uncertainty in closing the left eye. On the right side there was ptosis. The face was drawn to the right side, but the tongue was protruded straight. The hearing was doubtful. The eyes showed no signs of previous iritis, nor did the ophthalmoscope discover any syphilitic lesion of the fundus, while the sight was apparently good and equal.

The right hand, arm, and leg seemed normal in sensation and movement.

The left hand and arm were limp. The grasp of the left hand was lost, but she could raise the wrist slightly if the supinators were in extension. She had some power over the supinators, and could partly flex the fingers. She had considerable power in the biceps, but apparently none in the triceps. Pectoral muscles good. Sensation much impaired in forearm, more so in hand, and in both situations slow telegraphy.

The knee jerk was absent in both legs, and there was slight clonus. The right leg was otherwise fairly normal, the left impaired as regards sensation and movement, but capable of fair resistance. The paralysis was not nearly so marked as in the arm.

There was complete incontinence and unconsciousness as regards the bladder, less marked with the rectum. No cerebral macula.

The appetite was large, and there was no vomiting.

The urine had a sp. gr. of 1.026. Contained a trace of albumen, but no casts. Deposited urea in crystal when treated with nitric acid, and did not contain sugar.

No sign of syphilitic scar could be discovered, but there was a history of four miscarriages, and of some partial paraplegia having existed for two years after a bad miscarriage. At a later date there was obtained a clear history of infection twenty years previously with sore throat and slight skin rash.

The paralyzes scarcely fitted the diagnosis of cerebral tumour, and certainly were not those of fine nerve lesion. An

arterial lesion, with or without gumma, seemed more probable. Note the result of treatment. The nape of the neck was blistered, and 120 grains of iodide of potassium given daily, with the result that in ten days' time the patient was practically well, and only required to be told to continue her iodide in reduced doses so long as she improved in weight. It is now nine months since this incident, and she remains well.

The symptoms which preceded this cure were obviously due either to pressure on brain substance or to starvation of blood supply. How different is the history when the nerve tissue proper is involved in a finer lesion. In these cases the symptoms are ataxic, choreic, or affect nutrition; and of these three, the latter is the most hopeful.

CASE II.—A man, aged 33, had primary syphilis in 1867, but no secondaries. Two years later had slight secondaries, lasting only a few days. In 1880 he had two attacks of remittent fever, which left him with slight torticollis, a little difficulty in writing, and an inability to concentrate his attention on his work. He was abroad at the time, and was invalided home for climatic debility. He had lost 30 lbs. in weight, and was liable to faintness without loss of consciousness. These symptoms disappeared during a six months' residence at home, and without anti-syphilitic treatment. He applied to be employed again, but was refused, as showing signs of past sunstroke. I saw him in May, 1881. He had then evidence of ataxy with abolished knee jerk, springy sensation in walking, total impotence, and spasmodic torticollis. Dr. Buzzard saw him later, and developed the diagnosis of syphilis.

In this case the most strenuous anti-syphilitic treatment, combined with or alternated with tonics, has been quite useless. The progress of the ataxy has been uninterrupted. Nor would it be difficult to add other cases.

CASE III.—A young medical officer, aged 30, was invalided home for Peshawur fever, and was poisoning himself with quinine. He was reduced to a skeleton, had slight rises of temperature at odd hours, and was utterly miserable and hopeless. I had some doubts as to the fever, as I found that his left arm and leg were atrophied, and I put him the question, "Might it be syphilis?" His medical history sheet recorded an infection in the finger, which had been treated. The symptoms of fever disappeared under 10 gr. doses of potassic iodide three times a day, and the nutrition of the

limbs visibly improved. There is some hope that in his case the development of true nerve syphilis will be delayed.\*

One cannot fail to be struck by the obvious questions to which such cases give rise; and if it be true, as I believe it to be, that mere trophic lesions are more susceptible to treatment than those which are indicated by ataxic or choreic symptoms, the questioning may be pushed a little farther. A mere trophic lesion may not, after all, be more than a starvation of blood supply. An ataxic or choreic lesion may mean amyloid change in the nerve substance itself, and such change in any structure is peculiarly insusceptible to treatment. But it must be remembered that these are questions rather than replies, in the same degree as the cases are illustrative rather than exhaustive. To accumulate cases is rather the province of the specialist than of the general practitioner. To most of us it falls to have our study of disease biassed by the observations of the consultant, and in the work, and too often the worry, of general practice, to endeavour to supplement what he can tell us by more prolonged personal observation.

## A SHORT PAPER ON THE USE OF THE MORE COMMON ANÆSTHETICS.†

By J. ANDERSON ROBERTSON, M.A., M.B.

So much has been written and said about anæsthetics, and so commonly are they now used, that some gentlemen present may wonder at me for having chosen such a subject for a paper.

Much, however, both of what has been written and said, is absolutely wrong; and no little ignorance still exists, even among many members of the medical profession, regarding the use of these compounds. This ignorance is undoubtedly due to the absence of opportunity of acquiring a practical knowledge of their administration.

As the law at present stands, no man may become a qualified practitioner unless he can show a certificate to the effect that he has had practical experience in vaccination—an operation so simple that I have seen an old untutored woman vaccinate dozens of persons during a small-pox scare,

\* I have just heard that this case has relapsed, probably from want of perseverance in treatment. He is now unable to feed himself.

† For discussion, see page 284.

and vaccinate them as successfully as any public vaccinator; and yet the majority of men are launched into medical practice without once having administered an anæsthetic.

A *theoretical* knowledge all are supposed to have; but I hold that such a knowledge without practical experience is worse than useless, for it simply fills men's minds with misgivings, and causes them to dread the agents which the law qualifies (?) them to use (or misuse). Besides, the so-called theoretical instruction is often incorrect, and it by no means presupposes a practical knowledge on the part of the instructor.

I can remember well the wicked feeling of triumph I once experienced in answering the following question, which was put by an examiner who, some few weeks before, had given chloroform to a gentleman whom he knew to be suffering from a weak heart, and who never awakened from the anæsthesia. The patient was *stuck upright in a chair* for a trifling operation, and, humanly speaking, this position was the cause of death. The matter was very properly kept quiet, and I was not supposed to know anything about it. "How would you give chloroform?" said the examiner in question. "In the first place," I replied, "I will tell you how I would not give it. I would not give it to a patient in the upright position, more especially if that patient were the subject of heart disease." I was not allowed to say any more, but was at once passed on to the next examiner. The former examiner was not, I may mention, a surgeon, but he was a highly respected (and deservedly so) family physician, whose practical experience of anæsthetics, however, must have been very limited, whatever his theoretical knowledge may have been.

Again, I question very strongly the accuracy of the statements made regarding the so-called physiological actions of various anæsthetics. One reads in books of the various stages of anæsthesia—of how the influence of the drug invades successively the different regions of the brain; that during one stage we may expect struggling, and so on. Now, with regard to struggling, I may briefly say that in the vast majority of cases there will be no struggling at all if the drug be properly administered, and that, where it does occur (as, *e.g.*, in the case of obstreperous boys, hysterical persons, and drunkards), it is not usually at the time when one would, from written accounts, expect it. In the first two classes of persons the struggling almost invariably begins with the first inhalation of the drug (sometimes even prior to that); and

in the case of drunkards it persists more or less until the patient is profoundly under the influence of the anæsthetic, and sometimes throughout anæsthesia. In nothing is the truism, "Practice is better than theory," more true than in the giving of anæsthetics, in which an ounce of practical experience is more valuable than a ton of theoretical instruction.

It is, I hold, a burning disgrace to the system of medical education as carried on in this country, that so many men should be allowed to graduate without once having given an anæsthetic. No wonder that so many practitioners have a horrible dread of anæsthetics, when all practical experience in the giving of them has to be obtained after one settles down into private practice. Every man ought to hold a certificate of competency in the administration of these agents; but to make this possible it would be necessary to put an end—at once and for ever—to the enormous clinical classes, which obtain in—*e.g.*, such a hospital as the Western Infirmary. Too much theory, too little practice, is the crying evil of the present system of medical instruction; and such will continue to be the case, so long as clinical teachers are allowed to gather around them classes of such magnitude that real practical instruction is an impossibility. *χειρουργία* means "a working by hand," and students of surgery used to be taught to use their hands. Now-a-days, it would take up too much of the teacher's time, and be too much trouble to him to instruct his pupils in the use of their hands, so he gives them the *γνώσις*, or theoretical instruction, and expresses astonishment when at examination the *χειρουργία*, or practical knowledge is sadly wanting.

The rule which says that no man may give chloroform or other anæsthetic in a hospital, unless he be a licentiate of some medical school or a graduate of an university, is a most iniquitous and mistaken one. It is iniquitous, because it altogether takes away from the clinical instructor, the power of giving practical tuition to those who look to him for such instruction, and it is mistaken, because it assumes that the mere obtaining of a general qualification presupposes a practical knowledge of anæsthetics.

Students of medicine have a right to expect—Nay! to demand of their clinical instructors ample practice in the giving of chloroform, &c., and, although the imparting of such instruction would entail a good deal of extra work on the part of the teacher, yet it is his bounden duty to give it, both in the interests of his pupils, and of those to whom they may be called upon to administer such drugs in after life.

It is all very well for the managers of a hospital to say, "We will not place the lives of those patients who come here in jeopardy by allowing others than graduates to give anæsthetics;" but this is simply a lame way of going round a difficulty instead of honestly facing it; and, what is worse, it is a gross misstatement in point of fact. It certainly causes ignorant subscribers to exclaim—"Quite right and proper," and to extol the management for their kindness of heart in preventing a "Bob Sawyer" from doing to death the poor patients of a hospital. That this is a misstatement on the part of the directors is proved by the fact that in other hospitals where students give anæsthetics, the deaths attributed to such drugs are not more numerous than in those *well governed* ones, such as I have mentioned. Further, *it is dishonest*, for it leads an ignorant and confiding public to imagine that the qualified residents have necessarily had practical experience, a thing which none of them can have had, if their clinical instruction has been obtained in such a school. Clinical instructors may *connive at*, or at least *acquiesce in*, such an arrangement, because it is one which allows them to shirk a most important duty; and which, further, does not interfere with their having unwieldy and unteachable classes; but it is in direct antagonism to the best and highest interests both of teacher and taught.

If every student of surgery were obliged to give chloroform to a certain number (say 20 or 30) of patients before being allowed to enter for his final examination, and in that examination had to exhibit an ordinary amount of skill in the giving of it, the death-rate attributed to anæsthetics would speedily be reduced to a minimum, and both practitioners and the public would reap great advantage.

To show the inutility and absurdity of such a rule as that insisted on by the directors of the Western and some other infirmaries, I may mention that not very long ago I chanced to see a qualified resident in a hospital where such a rule obtains give chloroform to a patient. So carelessly and ignorantly was this done that I could not help remarking to a gentleman who stood looking on—"If that young man does not give a patient his quietus some day it will not be his fault!" On the very next morning this graduate had the misfortune to lose a patient under chloroform, and, of course, the chloroform got the blame. Now, I do not mean to insinuate that this death might not have occurred in the hands of any chloroformist, however experienced; but the want of practical experience and of the careful watchfulness which this experi-



ence begets, are certainly large factors in bringing about fatal anæsthesia.

To come now to the various compounds generally used as anæsthetics, I will merely mention the three most commonly used—viz., Chloroform, æther, and ethidene. Nitrous oxide I purposely exclude, as it does not satisfy the first essential of a safe anæsthetic—viz., the admission of atmospheric air to the lungs. It is simply an asphyxiating agent which, while in the majority of cases safe enough for minor operations, is certainly in many followed by disastrous results. I have investigated several cases of fatal broncho-pneumonia following the inhalation of  $N_2O$ , with the result that I consider it an asphyxiating agent less free from danger than any of the three anæsthetics named.

I may mention that I have used  $N_2O$  very frequently for minor operations, such as incising carbuncles, opening abscesses, removing teeth, &c., but extended experience does not confirm my early belief in it.

I have a grave suspicion that while  $N_2O$  fails to oxidise the blood—it being a chemical compound and not a chemical mixture like air—it nevertheless may sometimes take up more oxygen while being inspired, and thus come to form one of the highly irritating and poisonous higher oxides of nitrogen. If this be so (as I strongly think it is), we have a ready explanation of the causes of broncho-pneumonia to which reference has been made. In my opinion the most generally useful anæsthetic is undoubtedly chloroform; and most of the remarks I have to make refer especially to it.

Ethidene I have given about 200 times, and while it is less pungent than chloroform, and consequently more readily taken by children, it certainly is not safer. It is much more expensive; and, in mouth operations, where blood is necessarily swallowed, is succeeded by much more protracted and intractable sickness.

Æther I use very frequently—always employing the anhydrous sulphuric æther made by Macfarlane, of Edinburgh. This drug, when used alone, is a very valuable one; but its chief use with me is as the precursor of chloroform in cases of weak heart, or where patients have an inherent, though groundless, dread of chloroform. I may mention, in passing, that this dread nearly always has its origin in the misstatement of some timid practitioner, who, having a fear of administering anæsthetics, imbues his patients with a fear of receiving them. I have given chloroform to eight different patients of one physician, who owns he himself has never given it; and in each

instance the patient has been filled with the belief that he or she was sure to die if chloroform were given. This, of course, is an extreme instance; but every one who is constantly giving anæsthetics must have often heard the expression used—"I must not take chloroform, as my doctor says I am not a fit subject for it." But, to return, æther, while possessing several undoubted advantages over chloroform, has several grave disadvantages. Besides those usually mentioned in books I will merely mention the following:—1. It sometimes gives rise to severe conjunctivitis. 2. It often sets up bronchitis in the old or bronchitic; and this in spite of being warmed before inhalation. 3. It over-stimulates very excitable persons, rendering them often very noisy and obstreperous.

Chloroform is undoubtedly by far the most generally useful anæsthetic; and if a practitioner will only have confidence in his drug and in himself he need never fear to give it to anyone. Those who are vulgarly supposed to be the worst subjects for it, are assuredly those who need it most. Indeed, you may lay it down as a rule, almost without exception, that those with weak or fatty hearts, those with organic heart lesions, and those who are hypernervous, are the subjects most benefited by its use.

I have given it now between 3,000 and 4,000 times, to persons of all ages, and in every condition of disease, and I have never known any one to be injured by its use. I have never had to use tongue forceps, nor have I ever had to give a subcutaneous injection of æther, although I always have both forceps and æther at hand. On only one occasion have I been obliged to use amyl nitrite, and to have recourse to artificial respiration; and this was in the case of a lady who had to undergo an operation when far advanced in pregnancy, and who afterwards carried her child to the full term, both mother and child doing well after labour.

Although it may seem somewhat presumptuous in me to offer any suggestions as to the method of giving chloroform, as I have no doubt there are some gentlemen present whose experience of it is much greater than mine, still I may, I trust, be pardoned for laying down one or two rules which I have found useful in practice, and which are the fruits of my own experience.

1. Never give chloroform at an earlier period than four hours after easily digestible food has been taken.

2. Always examine the patient's heart immediately before giving it.

3. Ascertain by personal examination that every article of clothing about the chest or waist is quite loose.

4. Bare the neck and chest so that both pulsation and respiration may be easily seen.

5. Never give chloroform in other than the recumbent position.

6. Always have at hand a pair of tongue forceps, and a hypodermic syringe containing 10 or 15  $\text{m}$  æther; or, failing the latter, some amyl nitrite.

7. Always have a third person present.

8. Before holding the towel or inhaler over the mouth and nostrils, it is well to ask the patient to close the eyes, and to count slowly and audibly during the inhalation of the drug. It is also well to know the person's Christian name beforehand.

9. Never bring an inhaler or towel saturated with the anæsthetic suddenly down over the patient's face.

10. Always allow a sufficiency of air to gain access to the mouth and nostrils.

11. Watch most carefully the respiratory movements, and note the cardiac action by an occasional peep at the lips. Should the former become impeded or irregular, or if, when anæsthesia is well advanced, the patient should have greatly protracted expirations accompanied by groaning, stop the chloroform, raise the chin, and, by forcibly opening the mouth, bring forward the tongue. This, in my experience, has never failed to promote regular respiration, after which anæsthesia may be carried on.

Should the heart's action fail, as is always manifested by a sudden blanching of the lips, stop the chloroform; lower the head; inject hypodermically the æther at hand, and begin artificial respiration. A few drops of amyl nitrite applied to the nostrils will be found useful.

In the event of these means failing, I would endeavour to restore animation by *rhythmical compression of the heart*, combined with artificial respiration. This rhythmical compression has been frequently successful in restoring animals poisoned with chloroform; and, although I know of no case where it has been tried on man, I certainly would adopt it should the necessity ever arise. The compression of the heart causes a slight movement of the blood, and also acts as a rhythmical cardiac stimulus.

Böhm asserts that he has restored animation by this means in the case of cats after the heart's action had been arrested by chloroform for as long as 40 minutes.

12. The chloroformist should on no account allow his attention to wander from his duties; nor should he ever allow himself to be drawn into conversation by any looker on. The safety of the patient demands his whole and sustained attention.

13. Watch carefully from time to time the condition of the pupil, if the anæsthesia be profound, and the pupil have been contracted to almost a pin point, and then begin to dilate—the chloroform still being given, and the breathing diaphragmatic—at once stop the drug, and do not continue it until respiration is again quite normal, and the pupil again begins to contract. During the early stage of anæsthesia the centre for dilatation of the pupil (*viz.*, cervical-sympathetic, and trigeminus) is stimulated, so that the pupil dilates. With complete narcosis this centre is partially paralysed, so that the pupil no longer dilates. With more profound anæsthesia, this centre becomes completely paralysed, and the oculo-motor or third nerve comes into play, by which the pupil may be contracted to the size of a pin's head provided that the paralysis of the centre for dilatation be maintained by the drug being pushed. After a time the oculo-motor, or centre for contraction, becomes in turn paralysed, and rapid and complete dilatation takes place. It is at this stage that death rapidly ensues, unless means be taken to prevent it. Syncope occurs—caused by reflex stimulation of the cardio-inhibitory centre of the vagus, through the connection between the vagus and sympathetic in the cardiac ganglia.

This careful watching of the pupil I take to be one of the most important safeguards in the giving of anæsthetics, and it is because of the necessity for this that I have almost entirely discarded the use of the towel in administering these drugs. In place of the towel I use an inhaler made by Mr. Alis, of Philadelphia, which was obtained for me some  $4\frac{1}{2}$  years ago by the Messrs. Hilliard. The advantages claimed for this inhaler (and its claims I can fully substantiate) are:—

1. Its general utility:—It may be used equally well for æther, ethidene, or chloroform.

2. The saving of the drug.

3. The free admission of a good current of air. To my mind, however, its greatest advantage is that the eyes are quite uncovered, and are thereby relieved from the irritation of the drug used. But more important than this, it enables one to observe the pupil from time to time, without having to discontinue the giving of the drug. The condition of the pupil has in the past been much too little considered. Men

have been told to watch the pulse and the breathing. This is so far right; but in the pupil we have an index to both; and the careful watching of its changing size, with a clear comprehension of what these changes mean, will make any one who cares to study them a much more careful, and at the same time a much more confident anæsthetist.

If this short paper but serve to bring this point more prominently before the profession, it will not have failed of its purpose.

I may mention, in conclusion, that these remarks apply equally to the administration of ethidene and æther.

## CASE OF PULSATING EXOPHTHALMOS.

By HENRY E. CLARK,

Surgeon to the Glasgow Royal Infirmary.

(*Read at the Meeting of the Pathological and Clinical Society, Glasgow, 18th April, 1887.*)

THE patient, a woman of forty-six, having a reputation for intemperance (she says unjustly), had been taking a little whisky one Saturday evening, and on her return home stumbled as she was going upstairs, and struck against some object, which caused the infliction of a skin wound over the frontal bone of *left* side. She at the same time bruised her left cheek, and when seen by me a few days after, an ecchymosis was still present beneath her left eye. She was not aware of any injury to her *right* eye, nor was there any wound or contusion on the right side of the head. On the Monday following she noticed that the right eye had become bloodshot and swollen, was tending to protrude from the orbit, and the movements were becoming difficult and limited. The upper eyelid also drooped slightly. When seen by me, six days after the injury, there was marked exophthalmos, with ptosis, and complete fixation of the eyeball. The lids were swollen and œdematous, the veins of the upper eyelid were engorged, the conjunctiva was œdematous, so as to overlap the margin of the cornea, and the pupil was dilated and fixed. She said that her sight was good, and this was found to be the case as regards distant vision; but her accommodation was paralysed, so that near tests could not be accurately applied. There was no actual pain in the orbit, but there was a feeling of tension, and she complained of noises in the right ear, with slight deafness. There was marked pulsation of the orbital

contents, and on applying the stethoscope an evident and loud bruit, aneurismal in character, was heard. It has been noticed in some instances, where both eye and orbital contents are normal, that a bruit is heard when the stethoscope is placed upon the eyeball itself. Great caution was therefore exercised in this case that the stethoscope did not touch the eyeball, so as to avoid this source of error. When first seen, the ophthalmoscope could be easily used, and the appearances noted were extreme dilatation of the retinal veins, diminution in size of the retinal arteries, swollen, and oedematous condition of the disc, with haziness of the vitreous. In a few days, however, the cornea became too nebulous for the ophthalmoscope to be used.

While in this condition she was shown to the Members of this Society at a Meeting in October, 1885. She was kept under observation about a fortnight after appearing in this room, during which time the exophthalmos increased, the cornea became more opaque, and underwent superficial ulceration, which, however, only implicated its epithelial layers; the rectal vessels became very tortuous, and the tension of the eyeball increased. It was therefore decided to try digital compression of the common carotid, which was done for five minutes every three hours during four days—in all, for 24 hours, but without any appreciable permanent lessening of the pulsation. In a case reported by Vanzetti and Scaramoutzi, digital compression for five minutes at a time, five times in the twenty-four hours, continued for eighteen days, effected a cure, and we therefore thought it well to give this method a trial. As this failed, I performed ligature of the common carotid on 11th November. The operation was found to be a very simple affair, the sheath being got at from the inner side, the descendens noni nerve being recognised as it lay on the front of the sheath, and the opening being made on the tracheal aspect of the latter. A chronicised catgut ligature was applied, and the wound was closed with sutures of the same material, no drainage tube being used; the operation was performed and the wound dressed with due antiseptic precautions. Five days afterwards the wound was dressed, and was found to be entirely superficial. On the ninth day there remained only a small linear cicatrix. Throughout the progress of the case (both before and after the application of the ligature) the temperature was normal, the patient complained of no pain as the result of the operation, nor was there any cerebral or other disturbance. The ligature completely arrested the pulsation, together with the bruit and

tinnitus aurium, the congestion gradually passed off, the movements returned, and the exophthalmos became less, but it was noted nine weeks after the operation that the protrusion of the eyeball was still a prominent feature, and it was not till more than four months had elapsed that the eyeball regained its proper position.

The pupil remained dilated in spite of the use of eserine, and still remains so after a lapse of a year and a half. The lens became cataractous, and was extracted by me last summer. Vision in that eye amounts to ability to count fingers and no more. This diminution is, no doubt, due to secondary glaucoma, the symptoms of which were quite evident (cataract being one of them), and most of these symptoms still remain.

What was the pathological condition in this case?

The most complete study of this rare form of disease you will find in a paper, by Mr. Walter Rivington, in the *Medico-Chirurgical Transactions*, vol. lviii, p. 183. From the cases he has grouped in that paper, it appears that of traumatic cases by far the larger number were male patients, while in idiopathic cases females preponderated nearly in the proportion of three to one, and that in one-third of the females the affection came on during parturition. In very few cases has any aneurism been found, so that the old name, "Aneurism of the Orbit," is not a satisfactory title for the affection. In the majority of the few *post-mortems* recorded, the pulsating swelling in the orbit was formed, not by the ophthalmic artery, but by the varicose and distended ophthalmic vein, and that distention could be traced back to the cavernous sinus. Moreover, in six out of the thirteen recorded *post-mortems*, there was a communication between the carotid artery, as it lay in the cavernous sinus, and the cavity of the sinus; and in one case recorded by Nélaton this was the result of fracture of the base of the skull passing through the body of the sphenoid.

I think it is not improbable that my case was one of this nature, although it is true we have no collateral evidence of fracture of the base of the skull. To discuss this fully would, however, take up much longer time than I am allowed to occupy.

In conclusion, I may add that Rivington's table, showing the results of different modes of treatment, shows that of 44 cases treated by ligature of the carotid, 16 were cured, 6 benefited, 6\* died, 9 not improved, and 7 cured but with loss of vision.

\* Pyæmia, 2; secondary hæmorrhage, 2; cerebral disturbance, 1; not stated, 1.

## MEETINGS OF SOCIETIES.

## MEDICO-CHIRURGICAL SOCIETY.

SESSION 1886-87.

MEETING XIII.—15TH APRIL, 1887.

DR. PATTERSON, *Vice-President, in the Chair.*

I.—(a.) CASE OF EXCISION OF THE OS CALCIS; (b.) CASE OF CLUB FOOT IN WHICH A WEDGE WAS REMOVED FROM THE TARSUS; (c.) VESICAL CALCULUS REMOVED BY SUPRA-PUBIC OPERATION.

BY DR. RENTON.

The above cases were shown, and in reference to each a short paper was read, giving details of the operation.

*Dr. Beatson* said that he had lately seen a case of excision of the os calcis operated on by *Dr. George Buchanan* fifteen years ago. There was only a very slight lameness; and the movements of the foot were perfect. It was only after the lapse of a considerable time that the results of the operation could be tested.

*Mr. H. E. Clark* adverted to the importance of early operation in club foot, as generally the chances of obtaining mobility of the articulations were lessened in late cases. Removal of individual bones, especially the astragalus, was generally more successful than excising a wedge. In regard to the case of stone, he had in one instance removed an oxalate of lime calculus of very large size, performing the double operation. It was very difficult to get the bladder stripped off from the stone, to which it was firmly adherent. There was sometimes an advantage in the double operation from the discharge and the urine being drained off by the lower wound.

*Dr. Newman* said that he had lately been experimenting on the dead body, with the object of ascertaining whether in the supra-pubic operation it would not be possible to distend the bladder itself sufficiently instead of using a rectal bag. His method was to introduce into the bladder a balloon by a metal catheter. When the latter was removed, a tube connected with the balloon remained in the urethra. Into the balloon was now introduced a measured quantity of mercury, which dragged the bladder down, and the calculus was lifted up. The bladder was now distended with water or an antiseptic



fluid by the balloon. No urine or any fluid could escape into the abdominal cavity. He had had no opportunity of trying the method on the living subject.

*Dr. Fleming* doubted whether it would be wise to try the experiment described by *Dr. Newman* on the living subject. He spoke from some experience. Several years ago he suggested a procedure precisely similar, to be carried out on a patient of *Dr. Stirton*—a female with a greatly contracted bladder. The process of dilatation by means of the balloon was carried on till about five or six ounces of water could be injected; but it was carried too far, with the result of bursting the bladder. In regard to *Dr. Renton's* case of stone, he was surprised that he had not taken means to ascertain the size of the calculus. This should always be done, if it were possible. In a case of his own it could not be done, from the existence of an extremely tight stricture. He believed that the supra-pubic operation, with the drainage from below, with the incision of the membranous portion of the urethra, gave the best chances of success.

## II.—INFANTILE PARALYSIS OF THE RIGHT UPPER EXTREMITY.

BY MR. MAYLARD.

A case of marked infantile paralysis of the right upper extremity following measles was shown, and a demonstration given of the parts involved.

## III.—ANEURISM OF THE TRANSVERSE PORTION OF THE AORTA.

BY DR. NEWMAN.

A preparation was shown of an aneurism of the transverse portion of the aorta in which the affection had been diagnosed from the laryngeal symptoms. This was one of the cases referred to in *Dr. Newman's* paper read at the Society on 18th ult.

## IV.—TUMOURS OF THE PHARYNX AND LARYNX.

BY DR. MACINTYRE.

*Dr. Macintyre* read on "Tumours of the Pharynx and Larynx," and showed various forms of batteries and cauteries.

*Mr. Clark* said that the paper was really divisible into two separate memoirs, one on batteries and cauteries, and the other on the special subject of pharyngeal and laryngeal tumours. It was too late in the evening to discuss the materials in the paper with advantage. In regard to one

observation in the paper, ophthalmologists were anxious to impress on them that paralysis of the sixth nerve alone indicated intra-cranial disease, either cerebral or at the base of the skull.

*Dr. Newman* said that for a number of years he had been working at the attempts to employ electricity in surgery, and throat affections. He found that the great difficulty was to get the apparatus in proper order, so as to work the moment it was wanted. If it did so work its advantage was very considerable. With a good lime, or even gas light, they did not require electricity for operative purposes. But the electric light would be very useful for diagnostic purposes, if it were thoroughly amenable to control and therefore reliable. One point raised by *Dr. Macintyre's* paper was whether the removal of some portions of the tumour for diagnostic purposes was always permissible. If the growth was malignant it would probably do injury, unless the surgeon was prepared to perform extirpation at once. In the case of an epithelioma the disease would spread with great rapidity. An elderly gentleman came to him with a tumour, and the question was put to the patient whether, in the event of it turning out to be a cancer, he would submit to excision immediately. His affirmative reply emboldened him to remove a portion of the growth, which was found to be malignant. But the patient had in the short interval changed his mind, and declined the operation. Within a period of three or four weeks the glands became involved, the disease running an accelerated course. The intrinsic epithelioma had become extrinsic. As regards prognosis, it was important to distinguish between cancer and sarcoma. The order of tumours from a prognostic point of view was (1) extrinsic cancer, (2) intrinsic sarcoma, (3) extrinsic sarcoma, (4) intrinsic cancer. In the beginning of last year, he removed the entire larynx in a case of carcinoma, and the patient was up to the present time free from disease. It must be borne in mind the carcinoma was primarily a local disease, though arising from a constitutional peculiarity. The removal of the local lesion did not remove the danger of the disease occurring in the same or another part of the body; but the removal of a malignant tumour from the larynx involved a rather less chance of recurrence than in other parts of the body. The danger of the disease spreading below the vocal cords was reduced.

*Dr. Macintyre* thanked the Society for the way in which his paper had been received.

## MEETING XIV.—6TH MAY, 1887.

*The Vice-President, DR. JOSEPH COATS, in the Chair.*

## I.—CASE OF EXTERNAL BILIARY FISTULA.

By DR. W. G. DUN.

A large number of gall-stones removed, during life, by abdominal incision, were shown.

On 1st March, 1886, the patient whose case I bring before you to-night returned from work suffering from "severe pain in the stomach." I did not see him till a day or two later, but during this time the pain complained of had been constant, and there was sickness and vomiting. For some years he had been subject to similar attacks, which he always ascribed to biliousness. Although not naturally very robust, he has, with the exception of these bilious attacks, enjoyed very fair health. He is 47 years of age, and his occupation, which is that of a boltmaker, is of a very laborious nature, necessitating a constant stooping movement, and calling into action both arms and legs. When I saw him for the first time, I was much struck with the haggard, anxious appearance of his face, which had very much the aspect of a person suffering from malignant disease. I was, therefore, not at all astonished to find, on examining the abdomen, a distinct hard tumour situated above, and slightly to the right of, the umbilicus. It felt very firm, and could be freely handled without causing pain. There was no enlargement of the liver. The sickness and vomiting were not very troublesome, but most obstinate and violent hiccup came on, for which a number of remedies were tried, the most effective being chloral hydrate and bromide of potassium given in combination. At the end of about three weeks a discharge took place from the umbilicus, which, at first, was purulent in character, but later on of a dark brown colour, being, no doubt, largely composed of bile. On one or two occasions there was pretty severe hæmorrhage, requiring the application of ice. The discharge continued for some weeks, and as the man was evidently sinking, I plainly saw there was no prospect of recovery for him unless some attempt was made to ascertain exactly the source of the discharge and the nature of the tumour. I therefore asked my friend, Dr. J. Lindsay Steven, to see the case with me. On passing a probe through the opening at the umbilicus, it was found to go for some distance to the right in an upward

and backward direction, and on enlarging the opening, under chloroform, sufficiently to admit the forefinger, we discovered that it entered a cavity within which a considerable number of small bodies were felt, which were readily recognised as gall-stones. At the bottom of the cavity was a narrow opening leading to another cavity containing more gall-stones, and at the bottom of this second cavity another narrow opening could be felt. The outer space no doubt was an abscess cavity leading to the contracted gall bladder, within which could be felt the opening of the cystic duct. I removed altogether 56 small calculi, and subsequently another came away in the discharge. After this the discharge became very much more profuse, requiring the dressing to be changed twice daily, and often soaking through to the bed-clothes. It was largely composed of bile, the dressings being stained a yellow colour. A drainage tube was introduced, and the cavity washed out daily with carbolic solution. The discharge had never a fæulent or foetid odour. The drainage tube was from time to time shortened, and in the course of a few weeks the wound closed, but had to be reopened on account of severe pain, evidently due to retained discharge. I therefore purposely kept the opening from closing, and allowed the discharge to flow till about the beginning of July, when it had become very scanty, and ultimately ceased. The wound healed up entirely, leaving a large cicatrix, with somewhat of a tendency to a hernial protrusion. The patient had become extremely exhausted and emaciated, but he gradually regained strength, and is now nearly as well as ever. There is at present no tumour to be felt.

*Remarks.*—The case was one of external biliary fistula. Evidently the pressure of the distended gall bladder on the abdominal wall had led to the formation of an abscess, and the adherent gall bladder had later on ruptured into the abscess, which, in its turn, discharged through the umbilicus. The enlargement of the opening at the umbilicus, and the removal of the gall-stones, no doubt saved the patient, the discharge being allowed to come away freely, thus obviating the risk of rupture into the abdominal cavity and the occurrence of peritonitis, almost certainly fatal in the extremely exhausted state of the patient.

I have alluded to the patient's employment somewhat particularly, from the idea that the constant bending of the body and movement of the right leg, by hindering the flow of bile from the gall bladder, may have had something to do with the formation and accumulation of the gall-stones. The

situation of the tumour in the abdomen seemed to me much lower and nearer the middle line than one would have expected in the case of a distended gall bladder, where there was no enlargement of the liver, and this, with the hard feeling of the tumour and the rapid emaciation of the patient, led to the failure, on my part, of the early recognition of the true nature of the case. The movements of the body, incident to the man's occupation, led, I believe, to the adhesion of the distended gall bladder in this situation.

In conclusion, I ought to state that Drs. John Dunlop and W. L. Reid saw the case with me in consultation at an early stage.

*Dr. MacGregor Robertson* enquired whether the power of digesting fat was noticeably impaired? Was the opening to the cystic duct patent? Also as to the state of the bowels.

*Dr. Barlow* similarly enquired whether the bile found its way into the canal? Was there anything pointing to obstruction?

*Dr. J. Lindsay Steven* said that on seeing the case first it presented exactly the aspect of malignant disease of the stomach. The emaciation was great, and the man apparently had few days to live. Then came the exudation of pus from the umbilicus, without any appreciable opening, which threw some doubt on the nature of the case. *Dr. Dun* had clearly stated what was met with on making the incision—first an abscess cavity, and then the smooth-lined gall bladder, and after that either the cystic duct or the common duct. It was possible that the removing of the gall-stones may have opened up a duct previously impervious to the bile. Certainly the improvement after the operation was very marked and rapid.

*Dr. Coats* said that the case was interesting as exhibiting a condition of cachexia similar to that of malignant disease, showing that the latter was not of the essence of the disease, but the result of the draining away of material from the body. The cause of the great emaciation here was not very apparent. If it were the case that recovery occurred coincidently with a great discharge from the wound, then the rapid emaciation could hardly have been due to obstruction. It was probably caused by the formation of the abscess. The presence of inflammation, and the adhesion of the gall bladder to the abdominal wall required some explanation. He had never found gall-stones to produce localised adhesion.

*Dr. Dun* said that the patient's bowels were confined. As to the digestion of fat, there was scarcely room for observation, as the man took very little food of any kind, on account of the obstinate hiccup. To this cause, indeed, rather than to

others suggested, would he attribute the emaciation. Dr. Steven had rather overstated the fact when he said the recovery after the operation was marked and rapid. Some time after the operation his life was despaired of. The formation of the abscess was doubtless due to the fact that the man's frequent stooping, necessitated by his occupation, caused the gall-stones to exert pressure and set up a local irritation.

## II.—MULTIPLE ANEURISMS OF THE AORTA, WITH EROSION OF THE DORSAL VERTEBRÆ.

BY DR. J. LINDSAY STEVEN.

W. C., of middle age, was admitted to Ward XI, under Dr. Christie, on the 25th October, 1886, complaining of dyspnoea, cough, palpitation, and weakness. The man was moribund or almost so on admission, so that no clinical record could be made. On the 30th October I performed a *post-mortem*, and obtained the specimens which I now present to the Society. The first specimen consists of the thoracic aorta, which presents the following characters:—The vessel is dilated throughout, and presents very well marked atheromatous disease in its entire extent; and here and there the atheromatous patches have undergone calcareous infiltration. In addition to the aneurismal tumours, it will be observed that there are several little circular or oval depressions of the internal surface which may be regarded as the first step in the process of aneurismal dilatation. The vessel, as a whole, is a very typical illustration of the distorting influence of moderately severe atheromatous affection. It will be seen in the specimen that the innominate and the left carotid arteries arise by a common aperture, but I am inclined to think this is due to the dilatation of the vessel consequent upon the atheromatous disease. In addition to the changes just described, it will be seen that there are two well formed sacculated aneurisms situated in the course of the main trunk of the vessel. They are about the size of large walnuts, and are filled with firm laminated clot, and serve to illustrate the natural method of cure of such tumours. The upper of the two tumours was adherent to the inner surface of the upper lobe of the left lung, and was practically cured. The lower tumour was firmly adherent to the bodies of the vertebræ, so that its posterior wall was removed in taking out the aorta, and left sticking to the spinal column. The cavity of the tumour is seen to be filled with fairly firm clot.

The second specimen, which has been macerated, shows the portion of the vertebral column which was pressed upon by

the lower of the two tumours. It is seen that the whole of one side of the body of the 6th dorsal vertebra and portions of those above and below it, have been extensively eroded by the pressure of the aneurism. The characters of the specimen make it clear that we have here to deal simply with absorption of the bone by the continuous pressure. There is no evidence of any contemporaneous inflammatory reaction in the bone except at one or two points. At the junction of the 5th and 6th vertebræ two little stalletitic processes are seen, and a similar projection is observed at the upper margin of the 7th. This would almost seem as if in these situations there had been some inflammatory reaction. The appearance of the specimen also indicated that the intervertebral cartilages had not been eroded; and with regard to the other conditions there was slight granular degeneration of the kidneys and hypertrophy of the left ventricle.

In remarking upon the case, Dr. Steven referred to the frequency with which atheromatous disease of the arteries and granular degeneration of the kidneys were met with in the same patient, and threw out the suggestion that there might possibly be some causal connection between them. He illustrated his remarks by referring chiefly to the pathology of the so-called fibroid degeneration of the heart.

*Dr. Barlow* pointed out that in the specimen the vessels arose from the aorta by distinct orifices. The specimen also had a bearing on an anatomical question respecting the termination of the arch of the aorta, which, in text-books, seemed to have been descending for some years. The relative position of the arch to the ribs, as indicated by the specimen before them, suggested that the definite statements made in the text-books some dozen years ago were not far from the mark.

*Dr. Coats* said that he would have liked had Dr. Steven tackled the question how atheroma produced aneurism. It was difficult to give a very satisfactory reason why localised aneurisms could arise from an atheromatous condition of the vessels. His impression was that the manner in which atheroma did produce aneurism was from its destructive effect on the middle coat. The aorta was composed very largely of middle coat, and if there was no local interference tending to aneurism, he would attribute it to the effect on the middle coat. With this coat seriously weakened, any considerable increase in the pressure might lead to local dilatation. A general dilatation of the arch might also result from the propulsion of the blood from the hypertrophied ventricle into

the inelastic aorta. It was well known that when an aneurism projected against such an obstacle as the vertebræ, hæmorrhage was very apt to follow. The explanation was obvious. In the specimen before them the aneurism, at the point at which it came into contact with the vertebræ, was almost literally without any wall. The latter had, in fact, become atrophied from its being jammed by the impulse of the blood against the unyielding structures of the vertebræ. Hæmorrhage, therefore, was apt to occur. As had been pointed out, the intervertebral cartilages were not usually involved, and were generally found projecting from the eroded bone. Dr. Steven had started the speculative question of the cause of the connection between granular kidney and atheroma. In the same way, at a former meeting, Dr. M'Vail had speculated on the alleged connection of atheroma with emphysema. In both cases he (Dr. Coats) doubted the validity of the premises. He did not believe that there was evidence of such a connection and that the occurrence of the two conditions of atheroma and granular kidney, or emphysema, in one case was simply accidental. At all events, there was no visible nexus of connection; the diseases were different etiologically and pathologically, as well as from the clinical standpoint. The condition of the kidney known as granular was in no sense an atheroma.

*Dr. J. Lindsay Steven* said that he agreed with Dr. Coats as regards the absence of any connection between atheroma and emphysema; but he could not help thinking—with all deference to the opinion of such a distinguished pathologist as Dr. Coats—that an atheromatous disease of the arteries had some relation to a granular condition of the kidney; though, he admitted, that it might be difficult to produce very convincing proof of such a connection. They must keep in view that a condition involving extensive disease of the larger arteries would, in all probability, be accompanied by considerable disease of the smaller vessels and arterioles, and here they might possibly have the link of connection. They were aware that disease of the interstitial tissue of the heart, which led to fibroid patches, had been attributed to obstruction of the arteries. One of the best pathological authorities explained in the same way sclerosis of the spinal cord; and it was, at all events, quite conceivable that a similar cause might result in degeneration of the kidney. It was necessary to bear in mind the distinction between granular kidney and the subsequent tubular disease, with which he was not at present concerned.



III. — MICROSCOPICAL SECTIONS AND PREPARATIONS OF THE  
TISSUES OF A LIMB REMOVED BY AMPUTATION FOR INFAN-  
TILE PARALYSIS.

BY MR. A. E. MAYLARD.

Microscopical sections and preparations of the tissues were shown.

The patient was a girl, aged 18, who had suffered from paralysis of the right lower limb from below the knee since the age of ten months. She suffered much from chilblains every winter, and troublesome persistent ulcers on the dorsum of the foot. It was for this latter condition that the leg was amputated.

A subsequent examination of the limb revealed the following appearances of the parts:—

In external appearance the limb was plump and healthy, though lacking the solid feel of the opposite leg. On dissection there was found a dense and thick layer of subcutaneous fat, about half an inch in thickness, which almost uniformly surrounded the limb. On removing this the dense shining muscular fascia was seen, apparently unaltered in character. Beneath it posteriorly were found—the gastrocnemius, converted entirely into fat, with the exception of a few small patches of pink muscular tissue situated at what corresponded to its two heads; the soleus similarly, almost completely, converted into fat (what remained in the way of muscular tissue was found along its points of attachment); the popliteus showed, relatively to the two above muscles, much more normal tissue, but otherwise was largely converted into fat. The remaining muscles of the back, front, and outside of the limb showed no vestige whatever of pink muscular tissue. They varied somewhat in consistence and colour, some appearing white and fibrous, as if having undergone extensive atrophic changes without much fatty infiltration.

The bones (tibia and fibula) were much atrophied, the compact layer greatly thinner, and the cancellous spaces enlarged and filled with fat.

The microscopical sections were intended to illustrate the general appearance observed in the dissection. A section of the posterior tibial artery was shown illustrating a perfectly normal muscular tunic. Other sections also were shown illustrating the abundant vascular supply of the adipose tissue replacing the normal muscular fibre. The vessels appeared as the regular longitudinal meshwork peculiar to the method of supply of normal muscular tissue.

Mr. Maylard, in his remarks upon the case, said he thought the good vascular supply of the limb an interesting feature, and referred also to a case, which he showed at the last meeting, where a sphygmographic tracing taken from the radial pulse of a paralysed extremity was undistinguishable from that taken from the healthy side—two facts which seemed to prove that the non-development of a paralysed limb could not be accounted for by any want of proper blood supply, but solely by the abolition of nerve influence.

*Dr. J. W. Stark* mentioned a case of the kind, which he had seen, in which, along with the degeneration of the muscular tissue, there was a great diminution of the calibre of the blood vessels.

*Dr. Coats* presumed that Mr. Maylard, in saying that there was no decrease in the size of the vessels, referred to the arteries as seen in these microscopic specimens. If there was no diminution in the larger vessels, the fact would be anomalous, as the calibre of the blood-vessels was necessarily ruled by the requirements of the tissue, and a lessening of these requirements was necessarily followed by a diminution of the vessels.

*Mr. Maylard* said that he referred to the specimens; but on the point raised by Dr. Coats, it had to be taken into account that blood was required for the nutrition of fat.

*Dr. Coats* said that the supply required for fat was much less than that for muscle.

#### IV.—SPECIMEN ILLUSTRATIVE OF OSSEOUS MEDULLITIS.

BY MR. A. E. MAYLARD.

Mr. Maylard also showed a specimen illustrative of osseous medullitis. The specimen consisted of a part of the femur removed *post-mortem* from a patient whose limb had been previously amputated above the knee. Brownish pus exuded from the orifice of the lower surface of the femur, and a probe introduced at the exposed medullary orifice could be made to pass easily up to the trochanter. On making a longitudinal section of the bone, the whole of the medullary cavity was found to contain purulent material. The internal surface of the bone was in some places bare and necrosed, and the macerated specimen, as now seen, presents an almost completely detached circle of the internal layer of compact bone. For about a quarter of an inch external to this scale-like compact layer the osseous tissue is cancellous-like in structure, having undergone a process of rarefying osteitis.

*Dr. Coats* disliked the term "medullitis," which was hybrid, and preferred "osteomyelitis."

*Mr. Maylard* said that the term was used by such a great authority as *Ollier*, and he hardly thought that it was exactly synonymous with osteomyelitis, which involved the idea of an inflammation affecting the marrow and extending through the thickness of the bone.

*Dr. Coats* said that the prefix "osteo," he understood, was used to distinguish the affection from myelitis of the cord, not as implying that the bone was affected by the inflammation. In this sense he had always used it.

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MEETING XV.—13TH MAY, 1887.

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SIR G. H. B. MACLEOD, *President, in the Chair.*

I.—ON THE USE OF THE MORE COMMON ANÆSTHETICS.

BY MR. J. B. ANDERSON ROBERTSON, M.B. (See p. 262.)

*Mr. H. E. Clark* said that his attention had been directed to this condition of the pupil during anæsthesia when the memoir of *Budin* was put into his hands, and his opinion thereon was expressed in a review in the *Glasgow Medical Journal*. He was not, however, satisfied, and since that time in all cases in which he had administered chloroform he had carefully watched the behaviour of the pupil, and on the whole he was now satisfied that when anæsthesia was complete the pupil was always contracted, though he was aware that this did not accord with the views of other observers. *Dr. Neilson*, of the Glasgow School, who had recently returned from Vienna, had been there making a series of experiments on animals, the result of which he himself would make known, but he might state that one part of that gentleman's investigations had reference to strychnia as an antidote, and the results were very satisfactory, the administration of strychnia making the heart to begin acting after it had ceased for a considerable time. The mode of using anæsthetics ought to be taught to students by the clinical teacher. Of *Allis'* inhaler he strongly approved, finding it the most rapid, safe, and economical mode of applying anæsthetics. In the Eye Infirmary it sometimes happened that the enucleation of an eye occupied three minutes, counting from the beginning of the administration of chloroform. In small operations it was of great advantage to

be able to anæsthetise with rapidity. He considered that much more was to be gained by watching the pupil than by trusting to the indication afforded by anæsthesia of the conjunctiva, which was not a reliable sign.

Dr. J. Wallace Anderson said that it was noteworthy that in midwifery a much smaller quantity of chloroform was required than in ordinary surgical practice; a teaspoonful was often adequate. He was trained to believe in the use of the towel, but he was now satisfied that the inhaler referred to by Mr. Robertson was preferable.

Dr. Johnston Macfie said that from what he had seen, especially on the Continent, he was convinced that most of the mishaps resulting from anæsthetics were due to fear on the part of the administrator. In regard to the new rules in force in Glasgow, there was actually one institution in town in which no anæsthetic could be given unless in the presence of two members of the staff and of the house surgeon. This appeared to be the *reductio ad absurdum* of the new system.

Dr. MacGregor Robertson said that in giving chloroform he always used the towel, and had seen no reason to alter this practice. He did not think it was right to make experiments on human beings in the way of comparative modes of inhalation when the old method appeared satisfactory. He had made experiments in regard to the action of the heart under various anæsthetics. In these he took the isolated heart of a frog, which was put on a prepared stupe. He could stimulate the heart till it beat with extraordinary rapidity; on increasing the percentage of the agent, the action of the heart might be compared to the erratic motions of a drunken man, and still increasing the percentage, the heart was ultimately put to sleep, in which state he could keep it for twenty minutes, and then wake it up. Now, it was noteworthy what were the percentages of various anæsthetics required to produce respectively the conditions of rapid action, delirious action, and sleep. The percentages of ether were one, one and a half, or two per cent. As to chloroform, the difference was marked, the tendency of this agent being to throw the heart into a state of tetanic contraction. Under ether it occasionally happened that tetanus was induced; but under chloroform this was very much more marked;  $\frac{1}{20}$ th per cent of chloroform would throw the heart into very rapid action, while from '6 to 1 per cent would induce sleep. Ethidene, in regard to its action on the heart, occupied a position midway between ether and chloroform. As regards its administration, he would have no hesitation in giving chloroform singlehanded, as he had often done.

*Dr. Morton* said that about 24 minims of chloroform was usually adequate to produce insensibility sufficient for operations. About 4 to 5 per cent had been ascertained to be the amount of chloroform to be mixed with atmospheric air. During the period of 27 years of his connection with the Royal Infirmary, he had had no deaths from anæsthetics; and, in fact, he had never seen one. With *Dr. Murray* of Newcastle, he was of opinion that chloroform should be given rapidly, with the object of using less of it, and thus getting free of the cumulative effects of the agent in the system. The only times when the administration of chloroform gave him some anxiety was when he got a fresh hospital assistant, in whose over-caution he used to detect an element of danger. The changes in the colour of the lip were of more importance than the motions of the pupil. Pallor of the face and lips indicated the near approach of danger. He did not think that stertorous breathing was an indication to be attended to. In regard to the local effect of anæsthetics it was simply that of ice. It was numbness from cold.

*Dr. Turner* said that he had observed the fact alluded to by *Dr. J. W. Anderson*, that in parturition a small quantity of chloroform was sufficient. This was especially true after prolonged labour, and was probably due to the exhausted condition of the patient. He had noticed rapid anæsthesia from a small quantity of the agent in other cases—for example, those in which a person had lost a good deal of blood. His own experience in the use of anæsthetics was somewhat unique, he having been twelve years resident in the South Sea Islands in a district in which there was no other medical man. He had given anæsthetics hundreds of times to the natives, his assistant being a native whom he had trained. The operations lasted from a few minutes to over an hour. He had never had the slightest difficulty or mishap of any kind in that place; and it was only since he came home here that he had seen two cases in which dangerous symptoms were shown, though both recovered. Might not the use of impure chloroform be the cause of danger in such cases?

*Dr. Workman* asked whether *Mr. Robertson* had not been using *Allis'* inhaler upside down? *Mr. Robertson* said that he had let in air below, while the intention of the inhaler was to close out the air. Chloroform should be administered comparatively rapidly, to prevent the tissues becoming charged with the agent. Among the means of stimulating the action of the heart in cases of accident, *Mr. Robertson* had omitted to include electricity.

*Dr. Hugh Thomson* said that he was probably the first of the members present to administer chloroform, which he did, in Greenock Infirmary, not very long after its introduction as an anæsthetic. The teaching in these early days was that it should be given cautiously, with a sufficient admixture of atmospheric air. The only case in which he had seen any difficulty was one in which the patient was in a state of fear before chloroform was given. There was great diversity in the susceptibility to the action of chloroform, and consequent difference in the quantity required. He would proceed cautiously, if he had no knowledge beforehand of this factor of personal susceptibility.

*Dr. Richmond* said that he had lately been in Sir Joseph Lister's wards, and observed that he used a towel folded in the form of a triangle.

*Dr. Henderson* (Partick), said that having on one occasion inadvertently used, in a case of angina pectoris, a mixture of chloroform and of a lotion composed of sulphuric ether and benzoic acid, he observed that, far from doing harm, the mixture thus accidentally given, proved a rapid and safe anæsthetic. Since that time he had always used this mixture.

*The President* said that he had paid a great deal of attention to this subject, and held very decided opinions upon it. He would not enter on the discussion of some of the questions raised by Mr. Robertson, such as that of clinical teaching, though in that he differed widely from some of the views enunciated by him in the paper. He was old enough to remember the discovery of the anæsthetic powers of chloroform. He read his thesis on that subject, in which he collected all the fatal cases up to that date. He had personally tried various instruments in administering it, including the inhaler exhibited; he tried various agents besides chloroform for anæsthesia; it was in his wards that the experiments on various anæsthetics were made, by gentlemen selected for the purpose, by the British Medical Association. With this varied experience, he must admit that he was somewhat taken aback by a gentleman putting down his experience in figures representing thousands of cases. In the Western Infirmary he administered chloroform in all kinds of operative cases, even in some which might be called trivial. He made it a point not to refuse the use of anæsthetics even in cases in which such aid might be dispensed with. Over all the long period of his experience he never had one fatal case. One patient indeed died of an epileptic fit after chloroform was given; but the death was clearly due, not to the anæsthetic, but to

epilepsy. Sir Joseph Lister had a similar case. In his own patient there had been two epileptic seizures on the morning of the operation, but of this he was not informed at the time. He had many times got frights. Now, with all this experience, he never gave chloroform in any one year oftener than 400 times; and it was puzzling to be informed by a private practitioner, not very long in practice, that he had given it thousands of times. He differed from Mr. Robertson as to the best mode of administration. He had tried inhalers of various kinds, including that exhibited by Mr. Robertson, but he preferred the towel to any of them. But it must be properly folded, and must neither be too thin nor too old. The quantity of the chloroform put on the towel need not be stinted at first. The time which elapsed before the patient was anæsthetised was very brief. The patient might count from 15 to 20, rarely did he get to 30. The question of allowing students to administer the drug had been touched on. Some of his colleagues, he was aware, entrusted the duty to students; but he never could do so. He never even allowed his house-surgeon to begin the administration of chloroform. The danger, he held, was at the outset. When the patient was well under the power of the anæsthetic, then the towel was passed by him to his assistant. It might be urged, as had been done by Mr. Robertson that evening, that the students were not by this method practically taught the use of it. He, on the other hand, held that the lessons of personal responsibility, and of the gravity of the position of the administrator of the anæsthetic, could not be more effectually impressed on the minds of the students. Once fairly under the power of the agent, the lips of the patient must be carefully watched. The pupil was, no doubt, affected, but as an indication of danger the movements of the pupil were worthless, and if they leant on these they would trust to a broken reed. The attention of the person holding the towel must be given to the patient's face, and to nothing else. Any blanching of the face or lips, or laboured or stertorous breathing, was an imperative indication to remove the cloth instantly. He held that chloroform should be given slowly. The question of examining the heart had been raised. He held that this should not be done at the time, for the reason that it alarmed the patient, and needlessly raised in his mind the question of danger. The argument advanced that the operator would be in a better position in a court of law if he could aver that he had examined the heart, suggested the question—In what position would the administrator be in a law court if he

had examined the heart, found it diseased, and still gave the anæsthetic? It was right enough to examine the heart, but let it be done some days earlier. Personally, he would not be influenced by the condition of the heart, the really dangerous cases having been found to be those in which the heart was strong and healthy, and in which the administrator had been careless and off his guard. He thought it a dangerous thing, drenching the towel with chloroform several times. It was best to have plenty on the towel at first; and the patient could be killed as effectually with ten drops as with a hundred. Three classes of patients were dangerous subjects for chloroform:—First, the extremely nervous, who get into a condition of terror or become greatly excited; secondly, the intemperate; and thirdly, those who used tobacco largely. In regard to the alimentary canal, the bowels should not be empty, and, to secure this, some kind of easily digested food should be given some two or three hours previously.

*Mr. Robertson*, in reply, said that the paper had, at all events, served the purpose of evoking a discussion. *Dr. MacGregor Robertson*, who had admittedly never given chloroform otherwise than by the towel, was scarcely in a position to pronounce on the comparative advantages of different methods. *Dr. Morton's* doubts as to the value of the changes in the pupil would probably be dispelled had he made exact observations from day to day over a lengthened period. That anæsthetics, applied locally, acted in the same way as ice was not universally true. Chloroform, so applied, raised the temperature. *Dr. Turner's* fortunate experience, in the use of chloroform, among the South Sea Islanders, might be due to the absence, in his patients, of the deleterious constitutional effects of what was called civilisation. *Dr. Workman*, who had, admittedly, never used the inhaler, was not in a position to lay down the mode of using it; as a matter of fact, his suggestion on the point was erroneous. His other suggestion that electricity might be used to resuscitate the patient could seldom be acted on. It would be inconvenient to carry a battery when one was going to anæsthetise. It was even doubtful whether stimulation of this kind would do good. *Dr. Macleod* hinted pretty broadly his disbelief in the accuracy of his statement as to the number of times he had used chloroform. Now, several members there present were aware of the circumstances under which he had given the anæsthetic so often. For seven years and a half he had administered chloroform, and he certainly did not overstate the case when he said that he gave



it not less than 500 times yearly. He was well aware of Dr. Macleod's horror lest a death should happen from chloroform in his wards; but, as a student of his, he had to look at the matter from a different point of view, and he could say most emphatically that his students were dissatisfied in being excluded from practical experience in administering anæsthetics in his wards. If all clinical teachers acted in the same way, how could the students obtain any experience at all? As regards the examination of the heart, this could be done easily enough without alarming the patient; and if they were able to assure the patient that the heart was right, they could do nothing better fitted to quiet the fears of hyper-sensitive patients. If they found indications of disease, they could simply hold their tongue. He quite agreed with Dr. Macleod that it was a bad thing to drench the towel with chloroform after the patient had struggled for a time, and bring it suddenly over the face. The exhaustion of the patient consequent on the struggle constituted, in such circumstances, the element of danger. Nothing which had been said by any of the speakers shook his faith in the inhaler as the best mode of administering the anæsthetic, nor of the value of the motions of the pupil as a forewarning of danger. The lips were covered, and their blanching might thus escape observation till too late. The movements of the pupil could be watched from the beginning to the end of the process.

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## PATHOLOGICAL AND CLINICAL SOCIETY.

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SESSION 1886-87.

MEETING VI.—14TH MARCH, 1887.

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*The President, DR. JAMES FINLAYSON, in the Chair.*

I.—CASE OF SPINAL DISEASE.

BY DR. DUNLOP.

A case of spinal disease simulating pseudo-hypertrophic paralysis was shown.

II.—CASE OF CEREBRAL ABSCESS DUE TO EAR DISEASE.

BY DR. THOMAS BARR. (See previous issue, page 187.)

The temporal bone and the brain from a case of cerebral abscess due to ear disease were shown.

## III.—TWO CASES OF DISEASE OF THE BRAIN IN CONNECTION WITH EAR DISEASE.

PROFESSOR GAIRDNER and DR. JOSEPH COATS showed two cases of disease of the brain in connection with ear disease, in one of which there was an abscess in the brain, and in the other hemiplegia with peculiar localised lesions. (See Professor Gairdner's paper, page 241.)

## IV.—(a.) CASE OF BULBAR PARALYSIS; (b.) CASE OF SPASM OF THE DIAPHRAGM (?), WITH HEREDITARY TRANSMISSION TO FIVE CHILDREN.

BY PROFESSOR M'CALL ANDERSON.

(a.) S. H., æt. 45, shoemaker, came under my care on 1st March, 1887, on account of difficulty of articulation.

His family history is not good, as it appears that his mother died of phthisis, and he has lost nine brothers and sisters, the causes of their deaths being unknown, but there is no evidence of nervous troubles in the connection.

He has been somewhat intemperate in his habits, but there is no history or evidence of syphilis.

About three years ago, while sitting in his chair one night, he felt "curious," and asked his wife to give him a drink. She looked up, saw that something was wrong, and went to his aid just in time to prevent him from falling on the floor. He became unconscious, was generally convulsed, and frothed at the mouth, and, as the fit passed off, he fell asleep. Since then he has "never been the same man," being nervous, and his memory has been less retentive than formerly, but on this occasion there was no noticeable defect of speech.

Last July he had a second fit, similar in character, but his wife thinks more severe than the first, and on regaining consciousness, he could not speak at all. Under treatment, the defective speech rapidly improved, but was never completely restored.

About a fortnight before he came under my observation, after some increase of the difficulty of articulation for about three weeks, he had a third convulsive seizure similar to the others, but he was no sooner out of it than he took another.

Since then he has been dull, listless, and occasionally "pulled up short" when speaking, and looked vacantly before him as if he had lost the train of thought.

On examination, it was found that his memory was decidedly defective, and he complained of some dimness of

vision.\* There was no evidence of implication of the limbs, nor was there any laryngeal trouble, as the vocal cords moved freely on phonation. He had no difficulty of swallowing, but the palate seemed to be somewhat depressed, and the uvula rested upon the back of the tongue. He could hardly protrude the tongue beyond the oral aperture, and it was distinctly tremulous when he attempted to do so. He could not close the lips completely, especially on the left side, where there was some dribbling of saliva. He was unable to inflate the cheeks, and whistling was impossible. He had much difficulty in speaking, especially at times, and his speech was slow, and often indistinct, the words being "blurred."

From these symptoms it was inferred that the lower facial muscles, the hypo-glossal, and the glosso-pharyngeal nerves were involved.

Treatment consisted of faradisation of the affected parts, and the use of the bromides of potassium and ammonium, gr. x of each, in combination with small doses of iodide of potassium, and a bitter thrice daily.

He improved rapidly, so much so that, when shown to the Society on 14th March, the paralysis of the palate was almost gone, the tongue could be freely protruded, he could close the lips and inflate the cheeks moderately well, and he emitted a slight sound on attempting to whistle, while the power of speech was, in great measure, restored.

(b.) W. M., æt 43, fireman, admitted 25th February, 1887, complaining of weakness of the lower extremities, especially of the right, of seven months' duration.

His father is alive and well, his mother died at 40, of British cholera, one of his sisters of whooping-cough, and one, aged 40, "of cramp of the stomach."

From infancy he has been subject to attacks (about to be described), coming on, sometimes, three or four times in the course of a day, sometimes at intervals of two or three weeks. They most frequently occurred at night, especially after partaking of large draughts of cold water before going to bed, and were also liable to be induced by excitement.

During the last seven months the general weakness of the body which accompanies each paroxysm has become permanent in the lower extremities, but the paresis is slight, and does not

\* The following is Dr. Reid's report of the state of the eyes:—"Retinal vessels in both eyes diminished in calibre, more so in left than in right. Amblyopia for red. Nerves, specially the right, atrophied, and present a greyish aspect."

prevent him from walking, but renders him unsteady, and liable to fall on stepping on a stone, &c.

Each of the paroxysms above alluded to sets in with a general feeling of weakness, sometimes so great as to prevent him from moving, although he remains quite intelligent, and answers questions quite rationally. This is accompanied by an indescribable sensation ("feeling of heaviness") at the pit of the stomach, with inability to draw a deep breath, or to cough, but without pain or dyspnoea. The attack lasts from two or three minutes to a quarter of an hour, and he often perspires at its close. Shortly after the attack the general weakness in great measure subsides, except latterly in the lower extremities as above mentioned.

But the most remarkable feature in the case is that which relates to his own children. He has had eight, all of whom are alive. One of his daughters has enjoyed good health, one has nearly lost her eyesight (from cataract?) since childhood, one son has been an epileptic since childhood, and is an inmate of the Larbert Institution for Imbeciles, while the remaining five, four daughters and one son, have been subject to identical paroxysms to those experienced by himself, since childhood.

Professor M'Call Anderson concluded that these paroxysms were probably of the nature of spasm of the diaphragm, and although not epileptic nor belonging to the same species of disorder, might be regarded as first cousins to that disease. And in this connection, it is interesting to note that one member of the family has been epileptic since childhood, and that the treatment applicable to epilepsy, has already produced a remarkable amelioration of the spasmodic seizures.

The treatment consisted of—

|                      |   |   |   |   |                 |
|----------------------|---|---|---|---|-----------------|
| R. Potassii Bromidi, |   |   |   |   |                 |
| Ammonii Bromidi,     | . | . | . | . | aa ʒiv          |
| Potassii Bicarb.,    | . | . | . | . | ʒiss            |
| Tinct. Calumbæ,      | . | . | . | . | ʒiii            |
| Aquæ,                | . | . | . | . | ad ʒxii. Solve. |

Sig.—A tablespoonful thrice daily, along with faradisation of the lower extremities.

The great improvement which has taken place already (14 days) and which consists in the subsidence of the paresis of the lower extremities, and the marked diminution in the frequency and severity of the paroxysms leads one to hope that by perseverance, and increase in the dose of the bromides, they may be permanently arrested.

## MEETING VII.—28TH MARCH, 1887.

*The President, DR. JAMES FINLAYSON, in the Chair.*

## I.—CEREBRAL ABSCESS.

By DR. THOMAS BARR.

A BOY was shown who had been successfully operated upon by Dr. William Macewen for cerebral abscess. (This case has been fully recorded in the *Lancet* for 26th March, 1887, and the *Archives of Otology* for June, 1887.)

## II.—MICROSCOPICAL SECTIONS OF RODENT ULCER.

By MR. A. ERNEST MAYLARD.

The ulcer from which the sections were cut was a very typical one of its kind. It was situated at the lower part of the ear, having eaten out a concave piece from the lobule of the pinna and removed a small area from the cheek. The sections were cut in a progressive series—the first through the adjacent skin, the last well into the floor of the ulcer. Between these extremes the various stages of tumour growth are seen. That immediately external to the ulcer shows considerable proliferation of the epithelial cells, and that representing the edge of the ulcer, a small collection of cells, situated in a space quite distinct from, though definitely located in, the *rete mucosum*. Besides the excessive proliferation of normal epithelium, a large number of small exudation cells are seen in the immediate vicinity of the new growth, quite distinct, however, from the latter. As the floor of the ulcer is approached, this small collection of cells becomes gradually larger, nearing the surface and causing a corresponding disappearance of the surface epithelium, until finally the tumour tissue proper forms the surface or floor of the ulcer, covered by a layer of blood-stained exudation. The cells, which form the tumour, are much more deeply stained (alum-carmine) than the surrounding epithelial cells. They form variously shaped collections, but in many places these are so arranged as to form cylinder or columnar shaped masses, with intervening fibrous and cellular tissue. The nuclei are large and very granular; only at places does the protoplasmic surrounding become visible. It is also noticed that some of the collections have a distinct internal layer of "palisade cells," with also a limiting membrane. The minute structure

of the ulcer will thus be seen to resemble in every feature the drawings and description given by Dr. Thin in his recent work on the subject.\*

The sections cannot be said to throw any positive light on the still questioned seat of origin. But the fact of the tumour spreading superficially through the cells of the *rete* would seem to suggest that it probably arose from a structure in this neighbourhood—if not from the *rete* itself, very possibly from that extension of it which goes to form the external root sheath of the hair follicle or the sebaceous glands.

In support of the theory, however, that rodent ulcers arise from the glands, either sebaceous or sudoriferous, the definite lining of palisade cells with a *membrana propria*, seen in some of the sections, would seem to lend some weight. It must always remain a very difficult point to decide whence the growth arises. For, once having arisen, the primary seat is soon destroyed, and all that can then be observed is the mode of extension. It is a strange fact that, if the *rete mucosum* be the part from which the tumour arises, the cells do not show, in places, a tendency to form epidermic epithelium.

But there is nowhere the slightest evidence of formation of squamous cells, prickle cells, or "nests," and in this rodent ulcer shows a marked contrast to squamous-celled carcinoma (epithelioma). Again, the extremely slow growth of this ulcer would seem to render the formation of fully developed cells the more likely; so that one would have expected in rodent ulcer, more than in squamous-celled carcinoma, to find the cells becoming squamous if the *rete* be the source of the tumour. For these reasons, therefore, and the others above stated, I am inclined more to the view of the ulcer being of glandular origin, and arising rather from the sebaceous than the sudoriferous glands. My reason for preferring these glands rests on their anatomical position. Being situated in the corium, and so nearer the surface than the secreting part of the sweat glands, which are in the subcutaneous tissue, they would more nearly correspond to the advancing portion of the tumour growth which, as shown in the sections, is either in, or immediately beneath, the *rete mucosum*.

In many of the sections there exist large numbers of small cells, and these being mapped out so distinctly from the larger cells forming the tumour, it seems probable that they are merely irritative in origin, and have no specific relation to the tumour tissue proper. They resemble exactly those

\* *Cancerous Affections of the Skin.* George Thin.

so frequently seen in the neighbourhood of advancing carcinoma.

It is interesting to note the great amount of proliferation of epidermic cells in the immediate neighbourhood of the tumour; and although these must be young cells, like the advancing tumour cells, they are not nearly so deeply stained by alumcarmine as the latter. They must be simply an irritative overgrowth, caused by the advancing tumour tissue.

### III.—EVISCERATION OF THE EYEBALL.

BY DR. RENTON.

Two cases of evisceration of the eyeball were shown, the one with an artificial vitreous. Both cases illustrated the value of the operation as to movements of the glass eye used, and its better operation when compared with that in which the entire globe is removed.

Neither of the patients complained of any pain, and the uninjured eyes had improved since the operations.

Dr. Renton's paper, with notes of the above and other cases, will be published in an early number of the *Journal*.

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### MEETING VIII.—18TH APRIL, 1887.

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*The President, DR. JAMES FINLAYSON, in the Chair.*

#### I.—CASE OF INCIPIENT LOCOMOTOR ATAXIA.

BY DR. JOHN CARLYLE, Greenock.

R. S., aged 52, police officer, married, with a history of intemperance, none of syphilis, nor of family predisposition to nervous disease, had always enjoyed good health, with the exception of a varicocele that troubled him, and for which he had an operation performed in the Western Infirmary about four years ago.

About three months after this operation he began to experience a numbness and tingling in the lower extremities, and subsequently in the fingers, with sensation of coldness in the feet and knees. These symptoms have continued more or less, and now, in addition, he complains of a painful sense of constriction round the waist, in the groins, genitals, and in the legs.

He looks fairly well, although somewhat anæmic. The chest and abdominal organs are perfectly healthy. There

is no affection of any of the cerebral nerves. His pupils are normal and react to accommodation, but not to light. Ophthalmoscopic examination discloses no change in the optic discs, and he makes no complaint of weakness of sight. The chief symptoms are from the lower half of the body, and as regards sensation there are here and there over the lower extremities, diminution of tactile sensibility, analgesia of certain spots, and retardation of sensibility. The sense for temperature and weight is normal. There is an entire loss of myotatic irritability, as revealed by absence of the knee jerks. There are no marked trophic changes, although the muscles of the limbs are somewhat flabby. The functions of the bladder and rectum are not interfered with, but there is loss of sexual power.

This man's chief difficulty is on ascending a stair, when he has a tendency to fall backwards, and, although he comes down easier, he has to proceed very slowly and pay particular attention to his feet. He walks best on an even surface, and when he wishes to turn stops short for a second or two, steadies himself, and brings himself round with caution.

In this man's case there is no history of lightning pains—a common and early symptom in tabes, but the group of symptoms present are distinctive evidence of posterior sclerosis.

## II.—CASE OF PULSATING EXOPHTHALMOS.

By MR. H. E. CLARK. (See p. 270.)

A patient was shown on whom ligature of the carotid artery was performed.

## III.—BILIARY CALCULUS REMOVED FROM THE ILEUM OF A PATIENT IN WHOM IT HAD CAUSED INTESTINAL OBSTRUCTION.

By MR. H. E. CLARK.

The patient, a woman of 57 years, had been suddenly seized with pain in the region of the stomach, followed by vomiting, six days before admission into hospital. The obstruction was complete, and the distention of the abdomen considerable; percussion elicited that the colon was empty, and it was therefore thought probable that the obstruction was in the lower part of the small intestine. Abdominal section was performed by median incision, through an adipose layer nearly two inches in thickness; and just beneath the wound, in the middle line of the abdomen, the small intestine was found to



be obstructed by a hard, round mass so firmly impacted, that it could be moved neither upwards nor downwards in the bowel. On incising the bowel longitudinally, exit was given to the obstructing mass, which was found to be a gall-stone of considerable size. The bowel was stitched up by two lines of continuous suture; the rest of the abdominal cavity was explored for any further cause of obstruction, but none was found; and the abdominal wound was then closed by deep and superficial sutures. The patient did not rally from the shock of the operation, and died twelve hours thereafter. No *post-mortem* examination was permitted.

Dr. Newman examined the calculus, and found it to consist of cholesterine, with some fæcal matter; he gave its dimensions as follows:—Length,  $1\frac{1}{2}$  inch; width, 1 inch; circumference,  $3\frac{1}{4}$  inches; and weight,  $3\frac{1}{2}$  drachms. This seems a small stone to cause so complete an obstruction, but a reference to the literature of the subject shows that cases are recorded of even smaller stones causing fatal obstruction.

#### IV.—CASE OF PURULENT OTITIS.

BY DR. J. WALLACE ANDERSON.

I regret that I can say nothing of this case bearing on the pathological specimens shown by Dr. Newman. The patient was a soldier, æt. 22, and was admitted to the Royal Infirmary from Duke Street Prison, on 26th March, 1887. Besides being almost quite deaf, he was in a state of half stupor, and could give no account of himself. The most conspicuous symptom was considerable cedema of both legs. The face was pale, but not cedematous, and there was abundance of albumen in the urine. Later it was ascertained that he had been sent up from the prison as suffering from "bronchitis, Bright's disease, and purulent otitis." The respirations were rather hurried and laborious, and there were wheezing rhonchi in both sides of the chest, and a slight purulent discharge from both ears, but the case was considered to be essentially Bright's disease with anæmia, and was treated as such.

He made no complaint of pain; there was no vomiting or convulsions. The comatose condition became gradually more marked, and he died on the 1st April—six days after admission.

Dr. David Newman showed the petrous portions of the temporal bones from this case. There was suppuration in both ears, but no inflammatory lesion of the brain or its membranes.

## MEETING IX.—9TH MAY, 1887.

*The President, DR. JAMES FINLAYSON, in the Chair.*

## I.—LARGE AND VERY ROUGH CALCULUS.

BY DR. H. C. CAMERON.

A large and very rough calculus, removed from an old man this morning, was shown. The symptoms were of one year's duration, according to the patient's statement. It illustrated the fact that we might have a very large calculus with comparatively little suffering. The case also illustrated the great advantage of distending the rectum in the supra-pubic operation. This was only the second case he had done, and he was convinced of the value of distending the rectum. He could have performed the operation without distending the bladder at all, so prominent was it made by the rectal distension. The stone was a typical mulberry calculus weighing  $5\frac{1}{2}$  oz. avoirdupois.

## II.—A SPECIMEN OF PERFORATING ULCER OF THE DUODENUM.

DR. COATS and PROFESSOR W. T. GAIRDNER showed a specimen of perforating ulcer of the duodenum laying open an artery, and presenting during life hæmorrhage with other interesting clinical facts. (This case will be published separately, in detail, in a future issue of the *Journal*.)

*Dr. Macewen* asked if there were any adhesions in the case of gastric pyloric cancer which *Dr. Gairdner* showed; also what was the state of the physical examination in the case of duodenal ulcer?

*Dr. Gairdner* replied there were no adhesions, and that in the duodenal ulcer there was obscure resistance in the epigastric region, with well marked signs of dilated stomach.

*Dr. J. Finlayson* had a case some years ago in some respects similar to this. There was dilated stomach, resulting evidently from an ulcer. As the ulceration was recent, they were rather afraid to wash out the stomach; and this was ultimately stopped by the appearance of blood in the washings. Then they considered if anything in the way of Italian dilation might be attempted, as it was about the time that *Mr. Holmes'* papers were appearing. In considering the question, signs of more active ulceration revealed themselves in the shape of active hæmorrhage. At the *post-mortem* there

were found two ulcers—one on the duodenal, the other on the gastric side of the pylorus. Both were surrounded by a great mass of thickening, which might have suggested carcinoma if felt through the peritoneum. He thought that the operation of excision would have been a formidable one.

### III.—TUMOUR (NEUROMA) OF THE GREAT SCIATIC NERVE.

By DR. H. C. CAMERON.

The tumour was shown. The case had been seen by many leading surgeons; and the tumour—which was spindle-shaped and had grown from the substance of the nerve—had been removed by him some time ago. After the operation the cut ends of the nerve were united by sutures, but it was impossible to bring them closer than about an inch. The case had, so far, done well, but there was yet no return of muscular power.

*Dr. Macewen* said that it was interesting to him, as he had seen the case. He wished to know what *Dr. Cameron* had said, but the friends would not tell. He gave the opinion that it was a tumour either of, or in the immediate neighbourhood of, the sciatic nerve. He believed it to be a sarcoma either pressing upon or involving the sciatic nerve. He could give the friends no definite answer as to how much paralysis would remain after excision of the tumour, as that depended upon what would require to be done at time of operation.

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### MEETING X.—23RD MAY, 1887.

*The President, DR. JAMES FINLAYSON, in the Chair.*

#### I.—A CASE OF MYELITIS—RECOVERY.

By PROFESSOR M'CALL ANDERSON.

*Dr. Anderson* showed the case, with special reference to treatment. The man is 38 years of age, a tobacco manufacturer, and came under observation on the 28th February, 1887. Three days before admission he began to be troubled with an urgent desire to micturate, with inability to pass more than a few drops of urine at a time. This continued all day, and in the evening he was relieved by the catheter. Next morning, on attempting to dress, he found that he was losing the power of his legs. This enfeeblement continued to increase during the day, till, by night, he was unable to walk.

On examination, it was found that the paralysis of the lower extremities was complete, and there was an absence of sensation—as to touch, pain, and temperature—from the knees downwards. The knee jerks were absent. The bladder was distended with urine, and there was continuous dribbling. His bowels had not been moved for four days. He had a large superficial bed sore, extending over the greater portion of the back below the waist.

His treatment, in addition to attention to the bed sore, and to the bladder and rectum, consisted of the internal administration of liquid extract of ergot, in doses of from ʒss. to ʒi thrice daily, and the subcutaneous injection of from gr.  $\frac{1}{100}$  to  $\frac{1}{70}$  of sulphate of atropine twice daily.

On 20th May, before he left, the following note was taken:—"Patient now walks about all day. His gait is still a little uncertain, and he complains of a little uneasiness in the lumbar region. The sensation has quite returned, and the knee jerks are present. His bowels are regular, and he can keep his water, but not for a longer period than from thirty minutes to an hour.

In connection with this case, Dr. Anderson referred to another which was reported in the *Edinburgh Medical Journal*. It presented all the symptoms observed in the last, but he was in much greater danger, because sensation was defective up to the middle of the chest. The hands were numb and weak. (Dynamometer registered 35 kilogrammes on the left side and 17 kilogrammes on the right.) The breathing was laboured and noisy; he spoke with difficulty, and in a hoarse whisper; and dysphagia was so marked that an attempt to swallow fluids nearly choked him.

Treatment, which was commenced on the 27th of January, 1881, three weeks after the onset of the symptoms, was the same as in the last, but, in addition, flying blisters were applied to the spine, and as long as the dysphagia lasted he was fed entirely through the nose.

On the 30th March he was dismissed, practically well.

## II.—XERODERMA PIGMENTOSA.

By DR. THOMAS REID.

A boy, aged 9, was shown, suffering from epithelioma of the eyeball at the junction of the cornea, which he had removed a fortnight ago.

The tumour was flat, of a pale, reddish-white colour, and extended inwards from about the centre of the cornea to the

equator of the eyeball. When first seen, two months ago, the tumour was about the size of a pea, and situated at the corneo-scleral junction. There is also a freckled condition of the skin of the face and hands, with here and there little nodules, depressed in the centre, and with hard elevated margins.\* There was not much difficulty in removing the tumour, which was only slightly adherent to the sclerotic, but was firmly incorporated with the superficial layers of the cornea. After hardening in chromic acid, microscopic sections of the tumour showed "pearly nodes," with the columnar arrangement of the epithelium characteristic of ordinary epithelioma. Sections were also made of one of the nodules excised from the skin of the face, and these exhibited hardening of the epithelium, and in the condensed cutaneous tissues hair follicles and glands were embedded, with here and there a typical pearly node.

Regarding this condition of the skin, the parents state that it began when the boy was two years old—little nodules, the centre of each being cast off like "the core of a corn," so as to leave a depression. None of the other members of the family suffer from any affection of the skin.

### III.—CASE OF MULTIPLE ENCHONDROMA.

By MR. H. E. CLARK.

A case of multiple enchondroma in a boy 12 years of age was shown. (This will be published in full in a future number).

### IV.—CASE OF WORD-DEAFNESS.

By DR. DONALD FRASER.

There was exhibited the brain from a case of word-deafness, previously shown to the Society. Dr. Fraser described the speech defects of the patient, who had been under observation for about ten years, and who had been shown to the Society a year or so before his death.

The brain showed some degree of atrophy of the whole anterior portion of the left hemisphere. On inspection of the lateral aspect of the left hemisphere it was at once noticed that a marked depression existed near the posterior extremity of the fissure of Sylvius. There was a very distinct atrophy involving the posterior part of the superior temporo-sphenoidal convolution, and the posterior part of the supra-marginal convolution.

\* *Diseases of the Skin.* M'Call Anderson. Page 380. Xeroderma Pigmentosa.

*Dr. Barr* asked what was the condition of the hearing as regarded other sounds than those of speech, such as the tick of a watch.

*Dr. W. T. Gairdner* did not think that this case had received the attention of the scientific world that it deserved, and recommended a full record in *Brain*.

*Dr. Alex. Robertson* said that the condition of the angular gyrus was worthy of attention, especially in reference to the perception of words by sight. If the lesion extended to that point there should have been some word-blindness. For the angular gyrus, according to most recent observations, is associated with the perception of words by sight. He thought the case well worthy of record. According to *Dr. Fraser's* report, and also the observation of *Dr. Finlayson*, there was some defect in his power of recognising, or at least in naming written symbols. Perhaps the difficulty may be in explaining why this defect should be so slight, if there be, as there seems to be, a lesion of this gyrus.

#### V.—SUPPURATIVE DISEASE OF THE EAR ASSOCIATED WITH TUBERCULAR NODULES IN THE BRAIN.

BY *DRS. JOHNSTONE MACFIE AND DAVID NEWMAN.*

*Patrick M'C.*, æt. 5, was admitted to the wards on the 9th April, 1887, from the Royal Infirmary Ear Dispensary. His father states that three years ago he had measles, and ever since there has been a discharge from the right ear. The glands under the ear are enlarged; in front of it there is some cellulitis and a discharging sinus. The discharge from the ear is copious and purulent, but not specially foetid. The ear syringed out with warm boracic solution, and the sinus in front of the ear dressed with boracic lint. On the left arm there is a scrofulous swelling above the elbow that has discharged lately, but is now dry, and there is another that has not discharged on the right leg.

*19th April.*—Discharge from the ear is less and the sinus healing rapidly. Glandular swelling less. He complains of no pain, but there is now slight swelling and tenderness over the right mastoid. Air passes easily with the air douche, and besides the other means of clearing out the tympanum he is to have this repeated every second day. He eats and sleeps well.

*25th April.*—Four days ago it was noticed that on deep pressure over the mastoid, pus welled up from the ear. To-day a small incision was made in the usual situation over the

mastoid, and warm boracic solution was easily syringed into the middle ear and out by the meatus. Bare and rough pieces of bone were felt by the probe.

*2nd May.*—On the afternoon of the day on which the incision was made into the mastoid his parents visited him, and, unknown to the nurse, gave him a tart and some sweetmeats. From that time until the 30th he complained of some headache, vomited all solid food, the bowels were irregular, and he had a foul tongue. The temperature, however, never rose above  $99.4^{\circ}$ , and this was an evening temperature. He has been having small doses of Gregory's powder. To-day (25th April) he complains for the first time of pain at the back of the head. The vomiting is less. To have nothing in the way of food but milk at frequent intervals. The incision over the mastoid was slightly enlarged, so as to admit of the syringe being more easily used. Fluid passes freely out of meatus, and air passes easily with douche.

*5th May.*—Vomiting less, and retains more of the milk. Brandy ordered in small quantities with the milk. Bowels more regular (they were moved twice to-day), but tongue continues foul and red towards the centre; the temperature is normal. To-day he is drowsy, but not difficult to rouse; answers questions distinctly, and shakes hands when asked to do so. He says he has pain in the back of the head, and lies with his head thrown back. Some flattening of the right side of the face is noticed, and the mouth is slightly drawn over to the left. There is some drooping of the right upper eyelid, and he seems unable to close the eye completely. The discharge from the ear is rather less; no increase of swelling over the mastoid. Air douche to be discontinued.

*6th May.*—Patient died early this morning. Some hours before death the temperature rose to  $102^{\circ}$  F.; facial paralysis became more marked; he became quite unconscious, with dilated and inactive pupils, and some clonic spasms of the extremities before the end.

For a detailed account of the *post-mortem* appearances, we must look to Dr. Newman, but here I may state generally, that besides the suppurative disease of the middle ear, there was found to be acute meningitis, probably tubercular, and unconnected directly with the ear disease. The signs of meningitis were most recent and acute over the cerebellum. There were tubercular masses in both hemispheres, and distention of the ventricles with serous fluid.

What was of special interest with regard to the state of the ear was the fact that the roof of the middle ear was not closed

in by a plate of bone, but by membrane. Although, one might argue that this state of matters would much facilitate the spread of inflammation from the ear to the brain, there were no signs of it having spread by that way in this case.

We know that the roof of the tympanum is one of the points at which inflammation frequently spreads to the meninges; and what contributes to its readily spreading in this direction is a condition of the bone similar to that found in this case—the bony roof being absent or its place taken up by a cribriform plate. This state of matters is to a certain extent normal in young children, and is one of the causes that tend to increase the frequency of extension of the disease in them.

On reviewing the life history of the case along with the *post-mortem* appearances, I am inclined to conclude, that, although the tubercular masses in the brain, must have been present for a long time, the outbreak of the fatal meningitis was directly induced by what in other circumstances would have been a temporary congestion of the membranes produced by the sickness. The mistaken kindness of the parents in giving the boy a tart and sweetmeats disordered the stomach, caused sickness, and lighted up the fatal attack of inflammation.

*Dr. Barr* thought it probable that there was a causative connection between the inflammatory products in the cavities of the middle ear and the tubercular deposits in the brain. At the discussion on tubercle in this Society six years ago, it was held by Hamilton and others that the absorption of caseating inflammatory products might give rise to tuberculosis. This opinion was also held by Buhl, Niemeyer, and Cohnheim in Germany. I suggested at the discussion that in few parts of the body were caseating inflammatory products so frequently met with as in the cavities of the middle ear, and that tubercular deposition, especially in the meninges of the brain, may possibly arise at times from absorption of this caseous material. Besides, tubercle-bacilli have been found in these accumulations in the ear, and, just as bacteria may be absorbed from purulent collections in the ear, and excite septic purulent formation in the brain above, so may tubercle-bacilli be absorbed and give rise to tubercular depositions in the brain or meninges.

#### VI.—CHRONIC PURULENT DISEASE OF THE EAR WITH BASAL MENINGITIS.

BY PROFESSOR W. T. GAIRDNER AND DR. THOMAS BARR.

This case is related as Case I in Professor Gairdner's paper, p. 242.



## GLASGOW SOUTHERN MEDICAL SOCIETY.

SESSION 1886-87.

MEETING XII.—21ST APRIL, 1887.

DR. JAMES MORTON, *President, in the Chair.*

## I.—NOTES OF THE TREATMENT OF SOME CASES OF EMPYEMA.

BY DR. ROBERT POLLOK.

1. A pregnant woman was seized with pleurisy, which, in spite of treatment, was followed by effusion. She aborted, but was piloted through this difficulty, and paracentesis thoracis was subsequently performed, sixteen ounces of purulent matter having been withdrawn. She then made an excellent recovery. 2. A young man complained of dull pain in the posterior inferior region of his left side, which, on examination, was found to be due to effusion, although many of the classical symptoms of that condition were wanting. The patient slept and ate well, and was able to walk about. By paracentesis, twenty-five ounces of foetid pus were obtained, but erysipelas followed, and the patient died. In such cases, free incisions should be made and the pleural cavity washed out with carbolised water, free drainage being established. Sometimes patients with considerable effusion might attend to their usual avocations, as in case 3, where a telegraphist in the General Post Office, to whom the medical officer had refused invalidism, was found, on examination by Drs. Pollok and Finlayson, to be suffering from effusion. The patient was phthisical and had recently been operated upon for *fistula in ano*. Treatment by means of cod liver oil, malt extract, iodide of iron, and chloride of calcium, with three months' residence at the seaside, effected a cure. 4. A girl of eleven years had complete dulness of her left side, with cardiac displacement, symptoms which appeared within three weeks. Paracentesis was resorted to, and thirty-six ounces of serous fluid were drawn off, and the girl made a rapid recovery. 5. In a weak strumous child, of two years, effusion took place, causing considerable dyspnoea and cyanosis. After consultation with Dr. Gairdner, tapping was performed, and eight ounces of laudable pus were withdrawn. The fluid reaccumulated, and other four ounces were evacuated by a free opening, good drainage having been established and carbolised tow used as a dressing. The symptoms were relieved, and the child's

condition improved, but capillary bronchitis subsequently proved fatal. 6. Another case occurred in an engineer, aged 42; purulent effusion rapidly occurred and gave rise to distressing symptoms. On tapping, sixteen ounces of pus were obtained, and recovery slowly and satisfactorily took place. 7. Dr. Pollok saw the following case in Dr. Hugh Kelly's practice. The effusion gave rise to urgent dyspnoea and cardiac intermittency, the patient being of a distinctly phthisical constitution and history. The symptoms were markedly disproportionate to the amount of effusion, and hyperpyrexia was present. Without waiting for a subsidence of the temperature, paracentesis was resorted to, more for relief than cure. Twenty ounces of pus were first evacuated, and in a few days other ten ounces, and the patient obtained great relief. Four days after the tapping the patient died, after the occurrence of a sudden spasm. Dr. Pollok summarised the treatment as follows:—(1.) If the effusion be old and circumscribed, giving rise to no pressure symptoms or fever, change of air should be prescribed, along with antistrumous remedies, such as iodide of iron and chloride of calcium, in addition to the local application of oleate of mercury. (2.) If the effusion, whether acute or chronic, give rise to respiratory or cardiac embarrassment or septic phenomena, the fluid ought to be evacuated and this operation repeated, if necessary. (3.) If the fluid be found foetid, a free incision should be made, the pleural cavity washed out with an antiseptic solution, good drainage being established. In cases where any doubt existed as to the diagnosis, Dr. Pollok advocated the use of an exploratory puncture by means of the hypodermic needle. He thought paracentesis should be resorted to more frequently, and if done early, simple puncture might often suffice, and the more frequent use of the aspirator would lessen the number of cases of chronic encysted empyemic abscess, which may lead to disorganisation and atrophy of the lung, and require elaborate and dangerous surgical interference.

*Dr. Erskine* remarked that the treatment of empyema was not usually so simple as Dr. Pollok seemed to make out. Free drainage could not be secured by a simple puncture, although by means of two openings at different levels, the pleural cavity could be washed out. He regarded true empyema as purely a surgical disease requiring free incision and drainage, or resection of parts of the ribs.

*Dr. Steven* thought Dr. Pollok's experience of the treatment of empyema by puncture exceedingly fortunate. His own

experience of that disease was not large, but in all the cases he had seen there was the greatest difficulty in getting the wound closed, it having remained open for many months with a continual discharge. He considered resection most necessary in the treatment of empyema. Of course, he assumed that pus was present in all Dr. Pollok's cases, otherwise, they were not cases of empyema.

*Dr. Patterson* said, that for serous fluid in the pleural cavity, tapping with an aspirator is all that need be done, care being taken that air be not admitted. In empyema proper, particularly in tubercular or strumous patients, an opening from the lung into the pleural cavity is sometimes present. That cavity may be filled with pus, although no preliminary rigor has occurred, but only a rise in the temperature. In the case of a phthisical patient presently under treatment, he removed as much as three inches from three ribs. Simply tapping a case of this kind, he remarked, gives no chance of drainage, and resection of one or more ribs is necessary. This patient, he was glad to say, had progressed fairly well. He had also the following case under treatment:—A seaman had been working in his ship till a fortnight before examination, when it was found that air was present in the right pleural cavity, Hippocratic succussion having been easily made out. *Dr. Patterson* described his method of operating, taking care not to injure the intercostal artery, which is apt to glide away when cut. The cavity is washed out with a solution of carbolic acid (1 to 60), and a drainage tube inserted, to which is attached a large piece of wire to prevent it slipping into the wound. When the wound becomes smaller, a suitable drainage tube is inserted to which a piece of tape may be attached and tied round the body. A fistulous opening is apt to remain.

*Dr. Lapraik* had a similar experience to *Dr. Patterson*, and had not been able to get his cases off his hands so easily as *Dr. Pollok*. In simple cases of pleuritic effusion, tapping alone may be sufficient, but in empyema it has come to be regarded as almost necessary to remove a portion of rib. He had two cases in which resection had to be performed, and in one of them the cavity required draining for thirteen months before closing, but the patient now enjoyed perfect health. He was quite sure lives were being saved by that operation.

*Dr. Glaister* did not think that all *Dr. Pollok's* cases were cases of empyema, although his recoveries were certainly very good. His own experience was limited to three cases. In one case he made a free incision, and had no difficulty with

the drainage, and now the boy is quite well, the lower part of the lung, however, having become bound down. The question arose, how long after the onset of illness does the effusion become purulent? He believed that the purulent condition sets in with high temperature and rigors. He advocated early incision in all such cases before the lung becomes bound down. In one of his own cases, Nature had established two fistulous openings which gave free drainage. In another case, where pleurisy with effusion occurred in a man of 38 years of age, he made a free incision, and the opening remained patent for three months. He thought a puncture with the hypodermic needle a valuable means of diagnosis. Dr. Glaister also spoke of a similar case of his, regarding which a consultation took place, resulting in the decision to do nothing. The patient went to the country where a general practitioner made an incision into the pleural cavity, and evacuated a large quantity of pus, and the patient made a good recovery.

*Dr. Knox* stated that empyema is simply pleurisy which had become purulent. He remarked that it is sometimes impossible to diagnose empyema early. When the lung has become hepatized, it of course cannot extend to reach the chest wall.

*The President* drew attention to the case spoken of by Dr. Glaister as successfully treated by the country practitioner, and remarked that such generally self-reliant men are the best practitioners. He had seen a boy with an aerial fistula. In former times, empyema sometimes made an opening for itself. He remembered one case of abscess of the chest wall from which on opening both air and pus escaped. He regarded resection of the ribs as a barbarity, and unnecessary for the purpose desired. If the opening be made in the most dependent portion of the back near the vertebral column no contraction will take place along with the movement of the chest, and there will be no interference with drainage. Although the chest will fall in in the case of young children, it will not do so in older people, the ribs being ossified. By the operation of resection, the chest space of the patient is diminished, and even the expansion of the remaining good lung is interfered with. He had used the aspirator in such cases, but had not obtained such good results as Dr. Pollok.

*Dr. Pollok*, in replying, drew special attention to the necessity of early incision. He explained that he used the largest aspirating needle, and had also employed a trocar and canula, allowing the fluid to drain away.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

### MATERIA MEDICA AND THERAPEUTICS.

By Dr. A. NAPIER.

**Unofficial Formulary, B. P. C.**—A work of some importance, to the practitioner as well as to the druggist, has just been accomplished by a committee of the British Pharmaceutical Conference appointed in August, 1886; this is the preparation of an authoritative Formulary of Unofficial Remedies.

The intervals at which the *British Pharmacopœia* is issued are comparatively so long that many really valuable drugs are extensively used for years before they receive official recognition; and it is perhaps as well that it should be so, for no one would care to see untried remedies, or those which rest for their reputation solely on the advertisements of enterprising manufacturing firms, introduced into our national Pharmacopœia. But while drugs are being so tried, it is essential that the composition of their preparations should be uniform, and well known both to prescriber and to dispenser; they should, further, be as palatable and presentable as possible. It is to secure this very important end that this Formulary is issued, and, in taking this step, we in this country are simply following the example of the French and the Americans, who are already provided with such provisional formularies. The members of the committee hope, very reasonably it seems to us, to attain the ends they seek, and also to relieve us all from the incubus imposed by the use of remedies of secret composition or manufacture.

The formulæ published are not very many, only thirty-seven in number, but early additions are to be looked for. Most of them have been suggested by the various members of the committee; others are taken from the *U.S. Pharmacopœia*, from Martindale & Westcott's *Extra Pharmacopœia*, and from *Bailey's Physicians' Pharmacopœia*.

In order to indicate quite clearly in prescriptions that the preparations of this Formulary are ordered, it is suggested to the prescriber that he should add to the name of the medicine the letters "B. P. C." (British Pharmaceutical Conference.)

Should any of our readers have alterations or additions to suggest, these may be addressed to "The Hon. Secretary, Formulary Committee, 17 Bloomsbury Square, London, W.C."

#### LIST OF THE FORMULÆ.

(Weights and Measures, those of the B.P.)

1. Chloral cum Camphorâ. Strength, equal parts of each drug.
2. Elixir Cascara Sagrada. Strength, eight parts of liquid extract (B.P.) in twenty; dose, 15 m to 2 fluid drachms.
3. Elixir Guaranæ. Strength, four ounces of the powdered drug in one pint; dose,  $\frac{1}{2}$  to 2 fluid drachms. Guarana is a paste made from the seeds of *Paullinia sorbilis*.
4. Elixir Simplex. A pleasant flavouring agent or vehicle for nauseous drugs; dose, 20 to 60 minims.
5. Emulsio Olei Morrhux. Contains 50 per cent of Cod Liver Oil; dose, 2 to 8 fluid drachms.
6. Extractum Grindeliæ Liquidum. One fluid ounce represents an ounce of the powdered drug, the leaves and flowering tops of *Grindelia squarrosa* and *G. robusta*; dose, 10 to 30 minims. Used mainly in asthma.
7. Extractum Hamamelidis Liquidum. Made from the powdered leaves of *Hamamelis virginica*. Strength, same as last; dose, 2 to 5 minims. This

will probably take the place of "Hazeline," "Pond's Extract," and such proprietary preparations.

8. *Extractum Hydrastis Liquidum*. Made from the rhizome of *Hydrastis canadensis*. Strength, same as last; dose, 5 to 30 minims.

9. *Injectio Curare Hypodermica*. Strength, 5 grains of curare in one fluid drachm of water; dose, 1 to 6 minims.

10. *Linimentum Opii Ammoniatum*. Contains soap liniment, compound camphor liniment, and tincture of opium, of each, 6 fluid ounces, belladonna liniment and stronger solution of ammonia, of each, 1 fluid ounce.

11. *Liq. Ferri Hypophosphitis Co.* Each fluid drachm contains about 2 grains each of hypophosphite of sodium and calcium, 1 grain of hypophosphite of magnesium, and  $1\frac{1}{2}$  grains of hypophosphite of iron; contains no syrup or other flavouring agent. Dose,  $\frac{1}{2}$  to 2 fluid drachms.

12. *Liquor Picis Carbonis*. Contains four ounces of prepared coal tar with one pint of tincture of Quillaia.

13. *Pilula Ferri (Blaud)*. Each pill contains about 1 grain of ferrous carbonate; dose, 1 to 3 pills. This is made by Maben's process, by which the reaction which results in the formation of the ferrous carbonate takes place in the mortar. Some hold that the ingredients of the pill should be prevented from reacting on each other as far as this is possible, and that the formation of the carbonate should take place chiefly after the pill is swallowed, that the patient may have the benefit of a *nascent* preparation of iron.

14. *Pix Carbonis Liquida Præparata*; prepared coal tar.

15. *Syrupus Apomorphinæ Hydrochloratis*. Acid in reaction; strength,  $\frac{1}{4}$  of a grain in each fluid drachm. Dose,  $\frac{1}{2}$  to 1 fluid drachm.

16. *Syrupus Butyl-Chloral*. Strength, 16 grains in each fluid ounce. Dose, 1 to 4 fluid drachms.

17. *Syrupus Calcii, Manganis et Potassii Hypophosphitum*. Contains 2 grains of the first mentioned hypophosphite, and 1 grain of each of the others in a fluid drachm. Dose,  $\frac{1}{2}$  to 1 fluid drachm.

18. *Syrupus Cascara Sagrada*. Strength, 4 of liquid extract (B. P.) in 20 — i. e., half the strength of the elixir. Dose, 1 to 4 fluid drachms.

19. *Syrupus Ferri Hypophosphitis*. Strength, about  $1\frac{1}{2}$  grains in each fluid drachm. Dose,  $\frac{1}{2}$  to 2 fluid drachms.

20. *Syrupus Ferri Phosphatis Compositus*. Contains in each fluid drachm, about  $\frac{1}{2}$  grain of phosphate of iron, and  $\frac{1}{2}$  grain of phosphate of calcium, with small quantities of the phosphates of potassium and sodium. Dose,  $\frac{1}{2}$  to 2 fluid drachms.

21. *Syr. Ferri, Quininae et Strychninae Phosphatum*. Each fluid drachm contains 1 grain of phosphate of iron,  $\frac{1}{2}$  grain of phosphate of quinine, and  $\frac{1}{4}$  grain of strychnine. Dose,  $\frac{1}{2}$  to 1 fluid drachm.

22. *Syr. Hypophosphitum Compositus*. Each fluid drachm contains  $1\frac{1}{4}$  grain of strychnine and  $\frac{1}{2}$  grain of quinine, with 15 minims of the syrup of the hypophosphites of calcium, manganese, and potassium, and 30 minims of the syrup of the hypophosphite of iron. Dose,  $\frac{1}{2}$  to 2 fluid drachms.

23. *Tinctura Benzoini Simplex*. Strength, two ounces to a pint of rectified spirit.

24. *Tinctura Bryoniae*. Made from fresh root of *Bryonia alba*. Strength, ten fluid ounces represent one ounce of dried root. Dose, 1 to 10 minims.

25. *Tinctura Carminativa*. A pleasant flavouring and carminative agent, containing cardamoms, stronger tincture of ginger, and oils of cinnamon, caraway and cloves, with rectified spirit.

26. *Tinctura Convallariae*. Made from flower and stalks of lily of the valley, *Convallaria majalis*. Strength, two and a half ounces to one pint. Dose, 5 to 20 minims.

27. *Tinctura Coto*. Made from coto bark, "a bark of unknown origin, obtained from Bolivia." Strength, two ounces to the pint. Dose, 10 to 30 minims.

28. *Tinctura Ergotæ Ammoniata*. Ergot macerated and percolated with aromatic spirit of ammonia. Twenty fluid ounces represent ten ounces of ergot. Dose, 10 to 60 minims.

29. *Tinctura Erythrophlei*—Tincture of Casca. Made from casca bark, the bark of *Erythrophloeum guineense*. Strength, two ounces to one pint. Dose, 5 to 10 minims.

30. *Tinctura Eucalypti*. Made from the powdered leaves of the *Eucalyptus globulus*. Strength, four ounces to one pint. Dose, 15 minims to 2 fluid drachms.

31. *Tinctura Euphorbiæ Piluliferæ*. Made from the herb *Euphorbia pilulifera*. Strength, same as last. Dose, 10 to 30 minims.

32. *Tinctura Hamamelidis*. Strength, two ounces to one pint—i.e., one-tenth that of the liquid extract. Dose, 5 to 60 minims.

33. *Tinctura Hydrastis*. Strength, two ounces to one pint—i.e., one-tenth that of the liquid extract. Dose, 20 minims to 1 fluid drachm.

34. *Tinctura Iodi Decolorata*. The tincture is here decolorised by the stronger solution of ammonia. Strength, 250 grains in one pint; almost the same as the B. P. tincture.

35. *Tinctura Pruni Virginianæ*. Made from wild cherry bark, the bark of *Prunus serotina*. Strength, four ounces to one pint. Dose, 20 to 60 minims.

36. *Tinctura Quillaie*. Made from quillaia bark, obtained from *Quillaia saponaria*. Strength, two ounces to one pint. An excellent emulsifying and suspending agent.

37. *Tinctura Strophanthi*. Made from seeds of a species of *Strophanthus*, usually referred to *S. Kombe*. Strength, one ounce in one pint. Dose, 2 to 10 minims.

## SURGERY.

By MR. A. E. MAYLARD.

**Digital Amputations under Cocaine.** By W. L. North, Jun., M.D., of Brooklyn (*Annals of Surgery*, May, 1887).—The wound is first thoroughly saturated with a 4 per cent solution of hydrochlorate of cocaine before any examination is made, and then, after waiting a few minutes for absorption to take place, the wound can be examined and probed without causing any pain or inconvenience to the patient. The flaps may then be cut, following each considerable incision with a few drops of the cocaine solution, and also using it occasionally to wet the entire denuded part.

**The Surgery of the Lungs.** By Roswell Park, A.M., M.A., of Buffalo (*Annals of Surgery*, May, 1887).—The subject of this paper is discussed under three headings:—Pneumotomy, Pneumectomy, Thoracoplasty. Under the term Pneumotomy is included the opening by knife or cautery, and drainage of a cavity in the lung substance or connecting with its interior. It is indicated in the following conditions:—1, Bronchiectatic abscesses, and, 2, Tuberculous abscesses when they can be localised; 3, Gangrene of the lungs; 4, Pyo-pneumothorax; 5, Hydatid cysts; 6, Foreign bodies. Showing the results of operations for these various conditions, the author has collected in all 84 cases, and of these 28 were fatal—a mortality of 32 per cent. Pneumectomy implies resection of a part or whole of one lung. Block, at the Eleventh Congress of German Surgeons (1882), showed several living dogs which had undergone partial or total resection of one lung, thus demonstrating, so far as animals were concerned, the feasibility of the operation. Krönlein was probably the first to deliberately remove a portion of human lung, except for hernia of the same. His case was that of sarcoma of the ribs, when, after removal, a small nodule was discovered in the lung. This was excised with scissors, the patient making a rapid recovery. Weinlechner had to deal with a myxochondroma of the right chest wall, the complete removal of which involved an excision of a piece of lung. The patient recovered from the operation, but died later of septic pleurisy. Other cases are also narrated. The indications for the operation which the author feels justified in promulgating

are:—Hæmorrhage from a wound of the lung; hernia of the lung, traumatic; neoplasms, especially those of the pulmonary environment which involve the lungs; diseases (tubercular) of one lobe. Block attempted the operation for the last affection, removing both apices of a young lady supposed to have apical lesions. She quickly succumbed.

Thoracoplasty implies resection of one or more ribs, usually to admit of the thorax adapting itself to large cavities, the result of empyema. The author gives three cases of his own, two of which were successful. Little, however, need be quoted further of an operation which has now been so frequently practised, and with such good results, in this country.

**Urinary Incontinence of Children Treated by Anodynes per Rectum.** By Dr. Edward T. Williams.—“It is safe to say that the modes of treatment usually recommended for this distressing infirmity are frequently ineffective and disappointing. . . . For the past year or two I have been trying, with complete success thus far, the use of anodynes by the rectum, in the form of injections and suppositories of morphine, belladonna, or atropine.

“I find that morphine alone relieves for the time being, but does not cure. Belladonna and atropine are curative when continued long enough, though I find them to be better borne in combination with a little morphine, which counteracts some of their bad effects, and enables them to be given more continuously. Furthermore, the requisite dose of belladonna is smaller when combined with morphine. When these medicines produce headache or undue nervous excitability, I use the bromides as a corrective, or suspend their administration for a time. I have found no case where they could not be borne when properly given.

“As to the mode of administration, a 15 gr. suppository of cacao butter is most easily handled, and that which I prefer. They should contain a proper amount of extract of belladonna and morphine. For a child five years old, say  $\frac{1}{4}$ th of a grain of belladonna extract and  $\frac{1}{8}$ th grain of morphine; but the doses must be carefully adapted to the particular case in hand, beginning with a small dose, with a smaller relative proportion of belladonna, and increasing the latter and diminishing the morphine as toleration becomes established.

“If an enema or clyster should be preferred, it should consist of about a drachm of lukewarm water, with a few drops of atropia and morphine solution added, and administered with a small syringe.” (*Boston Medical and Surgical Journal*, 16th September, 1886.)—D. M'P.

**Surgical Treatment of Wounds of the Liver.** (*Centralbl. f. Chir.*, 29th January, 1887.)—Dr. H. Burckhardt, of Stuttgart, describes in full detail the history of a case of punctured wound of the abdomen, in which the patient was, when brought to the hospital, in imminent danger from internal hæmorrhage. When the external wound was enlarged, and the blood cleared out from the peritoneal cavity, a little searching discovered a deep incised wound of the liver, from which blood was escaping freely, welling out as it would from an incised angioma. This wound was stuffed with strips of iodoform gauze, the ends of which were brought out by the external wound. The ultimate result of the case was good, but the patient was for some days in considerable danger (*not* from peritonitis), and the healing of the wound was hindered for some time by the unfortunate overlooking of one of the strips of gauze, which was left in the depths of the wound in the liver. When this was discovered and removed the wound healed quickly and well. Dr. Burckhardt, for very good reasons, prefers “tamponade” to stitching for wounds of the liver; at all events, when the wound is anything but quite superficial. The consistency of the tissue of the liver does not favour stitching; and if the wound penetrates to any depth, there is certain to be free effusion of blood, and probably also of bile, into the wound for some time, even though the edges of the wound may have been brought together, with great risk of tearing out the stitches and supervention of peritonitis. The plugging compresses the



bleeding tissue, and closes any wounded bile ducts while the plug remains in ; or, if there is any oozing, the strips of gauze drain it outwards through the external wound, and by the time the plug is removed (in this case on the sixth day) there will be sufficient adhesion around the wound to prevent contamination of the peritoneum. Of course, when the plug is removed, a drainage tube must be inserted, for, as in this case, there may be a considerable flow of bile, even if there be no blood or pus, for some time. Dr. Burckhardt thinks that even when it is thought safe to use sutures, a drainage tube should be left in the external wound for a time till risk of oozing from the hepatic wound is over.—D. M'P.

**To Avoid Injuring Subscapular Nerves while Clearing out the Axilla.**—Prof. Dr. E. Küster (*Centralb. f. Chir.*, 18th March, 1887), shows that there is a risk of wounding the subscapular nerves during the operation of clearing out the axilla in connection with the removal of cancerous breasts, and so causing impaired movement of the shoulder joint. Experience has shown that, in consequence of this accident, the patient is unable to raise the arm completely, and even if this disability disappears, as it may in time, she remains unable to bend her arm behind her back. Küster believes that muscular degeneration, following the division of the nerves, is much oftener the cause of restricted movement of the arm than is the contraction of the axillary cicatrix, which is often supposed to account for it.

The muscles concerned are the subscapularis, teres major, and latissimus dorsi, which are supplied by the three subscapular nerves. The first nerve, the highest, is, from its position, out of danger. The second and third run outwards and forwards from their origin in the brachial plexus. The second courses by the inner side of the blood-vessels, giving off branches to the subscapularis muscle, and enters the teres major near its origin. The third runs along the lateral border of the scapula to about its middle, where it enters the latissimus dorsi muscle. Both the second and third nerves are apt to be cut, the second at any part of its course, the third where it passes from the bone to the muscle. If the third alone is cut, the latissimus dorsi alone is paralysed ; if the second also is cut, all three muscles are, but the subscapular only partially.

This injury may be avoided by the following method—Volkman's, modified in its latter stages. The operation, in which the incision is prolonged up the middle of the axilla, proceeds in the ordinary way, till the mamma is only attached by the axillary fasciæ. The skin is then dissected up from the anterior margin of the axillary wound till the muscular edge of the anterior fold is reached, and then the fasciæ, &c., are dissected off the anterior wall of the axilla. Behind merely the edge of the muscle, the latissimus dorsi is cleared, as it is by deep cutting from without in this region that the nerves are injured. Then the fascia is raised from below, and stripped up with the handle of the scalpel till the veins at the apex of the axilla can be seen, and then the finger nail is used till the veins have been cleared sufficiently to admit of their being clamped or tied, and divided. When this has been done the subscapular vessels and the second nerve can be seen deep in the wound, lying upon the subscapular muscle. The fasciæ, &c., can now be stripped from the posterior wall from within outwards without danger to that nerve, and a little care, when the edge of the scapula is reached, makes it easy to avoid the third nerve.

The axilla can by this method be cleared out without injuring any nerve except the intercosto-humeral. This is inevitably cut, but the effect of its division is merely a temporary disturbance of sensibility on the inner side of the arm.—D. M'P.

**Colotomy: A New Method.**—"Sonnenburg (*Berl. Klin. Wochenschr.* 6th December, 1886) has relieved patients suffering from carcinoma of the rectum by operating in the following manner. The abdominal wall was

incised in the linea alba, the peritoneal cavity opened, and the colon divided transversely above the point of disease. The lower end was then closed by suture, and dropped back into the abdominal cavity. The superior end was brought forward and fastened into the median incision, below the umbilicus. The advantages claimed for this method are, that the surgeon has an opportunity to examine the intestine in regard to the exact nature of the neoplasm, its situation or size, and to change the plan of operation to a more advantageous one, if necessary. The intestine is opened at a known point, and as low as possible, so that solid dejections are voided from the artificial anus. The contraction of the recti muscles serves to contract the artificial opening, and act as a substitute for a sphincter. The anus, in this position, is more conveniently situated for the patient than in the Amussat or Littri operation. Also, the irritation, pain, ulceration, decomposing discharges, and other complications arising from the access of feces to the intestinal pouch, situated between the seat of stenosis and the artificial opening is avoided. In case the rectum is wholly occluded, entering the lower end of the divided intestine into the wound in the linea alba is recommended. This gives an opportunity to keep this portion clear from collecting mucus or discharges." (*Boston Med. and Surg. Journal*, 24th March, 1887).—D. M'P.

**On Excision of the Knee.** By Professor Ollier (*Revue de Chirurgie*, 10th August, 1887).—The object of this paper is to point out a simple method of post-operative treatment, whereby consolidation of the parts is obtained under a single dressing. In operating, Professor Ollier makes an H shaped incision, the transverse part of the incision dividing the ligamentum patellæ. The lateral ligaments are preserved, and all the healthy parts of the periosteocapsular sheath, that is to say, all the tissues which tend to maintain the bones in position, and offer a means of forming ossifying material. The author lays much stress upon the complete extirpation of all pulpy membrane and tuberculous material, and also upon efficient drainage. In carrying out the latter procedure, drains are usually placed at each side of the part in the lateral incisions, and one always in the tricipital *cul-de-sac*. The bones are approximated and kept in position by means of wire sutures. Iodoform, to which the author attaches the greatest importance, is sprinkled freely over the surface of the wound, which is then covered with layers of carbolised gauze. The limb is immobilised by a plaster splint so applied as to allow of the dressings being removed, if necessary. If the treatment has been carried out efficiently, and no symptoms, such as rise of temperature or pain, manifest themselves, the first dressing can be kept on from 40 to 50 days. Two cases are described in full, illustrative of the method advocated.

**On Rhinitis.** By W. J. Penny, F.R.C.S. (*Bristol Med. Chirurg. Journ.*, June, 1887).—This short and serviceable paper draws attention to a class of cases frequently met with, but perhaps not so frequently differentiated. They are roughly sub-divided into those of:—

1. Irritable vascular hypertrophy of the mucous membrane.
2. Chronic fibroid thickening of the membrane.
3. Chronic catarrhal rhinitis.
4. Ulcerative rhinitis.
5. Polypoid rhinitis.
6. Arthritic and osteitic, combined with periostitic rhinitis.
7. Tubercular rhinitis.
8. Lupoid rhinitis.

A large majority of these are caused by neglected colds, traumatisms, an unwholesome state of the atmosphere, depending on the presence of chemically or mechanically irritating agents or germs, various constitutional conditions, notably struma and syphilis, sometimes gout and rheumatism. They are mainly kept up by changes of atmospheric conditions, injudicious diet, especially alcoholic excesses—and in these cases a very small quantity means

excess; in some cases lack of cleanliness, it may be meddlesome surgery, deformities of the parts, and other causes too numerous to mention.

In class 1 the treatment consists in using sedative remedies, soothing injections, such as warm boracic lotion, poppy head lotion, bathing the nose externally with the same, gargling the throat; and, after this is done anointing the nose inside and out with some sedative ointment—vaselin or lanolin, impregnated with cocaine, acetate of lead, belladonna, morphia, or eucalyptus.

In class 2 dilatation with bougies seems useful. A solution of chromic acid, applied after the parts are well douched and dried, accelerates the cure.

In class 3 powders are recommended, such as various combinations of boracic acid or acacia, with tannin. If there is any smell, iodoform is a serviceable adjunct.

In class 4 copious douches are useful and the application of some antiseptic ointment.

**The Radical Cure of Hydrocele.** By J. S. M'Ardle (*The Dublin Journal of Medical Science*, September, 1887).—The chief interest of this paper centres in the carefully collected series of cases to show the relative merits of the two commonly practised operations for this affection—incision and incision. The points to be taken into consideration are:—1st, The relative difficulty of the operative procedure; 2nd, the dangers attending them; 3rd, the length of time required for cure; and 4th, the chances of return after each method. With regard to the first point, the method of incision takes much longer, but the stitching of the tunica vaginalis to the skin can offer no difficulty to any surgeon. As to the dangers likely to follow on each operation, Dr. Otto Weiss has noticed in 115 cases of injection, 5 in which incision became necessary on account of suppurative inflammation of the tunica vaginalis and scrotal tissue. In 163 cases of incision collected by Kraske, only a single instance of suppurative inflammation was recorded. As to the third point, the time required for cure, the following figures show the relative date of healing:—

| Injection.         | Incision.                     | Excision.                    |
|--------------------|-------------------------------|------------------------------|
| Billroth, 9th.     | Volkman, 8th to 10th.         | Julliard, 10th.              |
| Stoltz, 9th.       | Küster, 14th.                 | Bergmann, 11th to 12th.      |
| Weiss, 8th to 9th. | Lister, 17th.                 | Author's cases, 7th to 12th. |
|                    | Author's cases, 12th to 14th. |                              |

The chances of recurrence in the different methods is shown to be 1 in every 8 of those treated by injection, 1 in every 47 of those treated by incision, while no recurrence can take place after excision.

**Upon the Question of Abortive Treatment in Gonorrhoea.** By E. Welander (*Centralbl. für Chirurgie*, No. 35).—Welander is of opinion that the gonococci multiply in the lining epithelium of the urethral canal, and only then penetrate into the lymph channels. He is, therefore, convinced of the possibility of checking the advance of the affection if treated sufficiently early. This he affects to do by first wiping the canal several times pretty forcibly with tampons of cotton wool, and then injecting a 2 per cent solution of sulphate of copper, allowing this to remain in the canal for some minutes. Welander has also sought to test the influence of sublimate solution upon gonorrhoeal pus. The solution was allowed to remain in the canal for some minutes, and then some of the pus was inoculated into a healthy canal. By using a sublimate solution of strength 1 to 1,000 or 1 to 5,000, the inoculation gave negative results; in the use of a solution of strength 1 to 10,000, in one case a positive result followed. Welander, therefore, deems this last solution, as also a  $\frac{1}{2}$  per cent solution of sulphate of copper, unsuited for abortive treatment.

**The Combination of Oxygen with Chloroform in the Production of Narcosis.** By Dr. Kreutzmann, San Francisco (*Centralbl.*

*für Chirurgie*, No. 35, 27th August, 1887).—Dr. Neudörfer, in Vienna, was the first to recommend the addition of oxygen to chloroform in the production of narcosis. The advantages which he claimed were absolute freedom from danger, a rapid production of anæsthesia, and the absence of the customary ill effects after general narcosis. While the best results appear to have followed the author's own practice of his principles, others, from various causes, have not found it possible to follow his example. The object of Dr. Kreutzmann's remarks is to show how this combination of oxygen and chloroform can be conveniently effected and how efficiently administered. An india-rubber bag containing oxygen is connected by means of a bellows arrangement with Junker's chloroform inhaler. In this way oxygen instead of air is pumped through the chloroform, and the patient breathes oxygen saturated with chloroform. It is to be noted that some air is always breathed, since complete exclusion of the same is not possible around the mouthpiece.

The author records 25 cases of narcosis produced by this method. In all the production of anæsthesia was very rapid; in none was there any special excitement exhibited. The patients rapidly came to, and no subsequent unpleasant symptoms—such as vomiting, &c.—ever manifested themselves. As regards the actual safety of the method, a much larger number of cases need to be recorded before anything definite can be said on that point.

## DISEASES OF THE THROAT.

By DR. J. MACINTYRE.

**Recurrent Naso-Pharyngeal Tumour—Cure by Electrolysis.**—Dr. Rufus Lincoln reports (in the *Polyclinic*, Philadelphia, June, 1887), the case of a patient suffering from a recurrent naso-pharyngeal tumour. Two needles connected with the negative pole of the battery were introduced through the anterior nares into the growth; the positive pole, terminating in two large sponge electrodes, was placed on the front and back of the chest. Six applications were made at intervals of four days. This caused an entire disappearance of the growth.

**Two Unique Cases of Congenital Occlusion of the Anterior Nares.**—Dr. Jarvis, of New York, reports two cases of the above. One, a young man, 18 years of age, with complete closure. Within the nares a cup-shaped depression of white glistening membrane was seen. On the left a small hole was seen. The second was a case of bony occlusion. (*Polyclinic. Report of the American Laryngological Association*, June, 1887.)

**Treatment of Laryngitis in Professionals who are unable to Rest.**—Dr. Solis Cohen, in opening a discussion upon this subject, at the American Laryngological Association, June, 1887, recommends the administration of an emetic, followed by rest, until the time of the performance, ice to be sucked and a cold compress kept to the neck meantime. This he finds useful where the voice is wanted in a few hours, and where hoarseness is complained of. Dr. Glasgow, of St. Louis, recommends the application of carbolised iodine to the larynx in this class of cases.

**Tubercle of the Tonsils.**—Dr. Lublinski reports two cases of this in the subsection of Laryngology (British Medical Association), August, 1887. He recommends the destruction, if possible, of such with the galvano-cautery. If no longer possible, he uses cocaine for the relief of the pain.

**Diphtheria in Man and the Lower Animals.**—Dr. Turner's report to the Local Government Board with regard to the connection between the infectious diseases of man and of the lower animals is of great interest. Dr. Turner does not say that diphtheria may not be spread mainly by personal

contact, bad sanitary conditions, &c. What he maintains is, that in other diseases, such as glanders and anthrax, the transmission from animals to man has been made out, and he draws our attention to certain facts pointing in the same direction in the case of diphtheria. Observations on the lower animals suffering from conditions resembling diphtheria and inoculations with the true diphtheritic membrane from a child's throat, producing in fowls conditions in every way comparable, are given. In view of the difficulty of explaining the outbreak in the ordinary way in many cases, Dr. Turner's evidence is exceedingly interesting and valuable.—(*Report of Dr. George Turner's Experience of Diphtheria*. A. & C. Black, Edinburgh; Eyre & Spottiswoode, London.)

## DISEASES OF THE EYE.

By DR. J. MACFIE.

**Suppurative Keratitis.**—(Noted in a lecture by Dr. Dudley S. Reynolds, in the *Medical and Surgical Reporter* for the 27th November, 1886). Here we note the chief points referred to. As to the severity of disease in his case, the lids were tumefied; the cornea throughout the anterior lower half grey; a mass of lymph hung from the anterior portion, which was ulcerating, and the anterior chamber was almost entirely filled with lymph. The author, who combats the general received opinion that such is a true hypopion, holds that, in these cases, the anterior chamber is filled with lymph and not pus, and writes:—"A condition, similar to that just stated, is described in all text-books as hypopion, and the older writers, even some at the present time, advocate paracentesis of the cornea, with a view of evacuating the irritating pus, as it is called. Some have even gone so far as to describe how the endothelial cells from the surface of the iris and cornea degenerate into pus and fall loosely into the anterior chamber producing this collection. This is a condition, the author states, that never exists in the form described; instead of pus, this accumulated mass of effused fluid, which presents a grey opaline, sometimes yellowish appearance, in the anterior chamber of the eye, is lymph, as may be easily demonstrated. No one will deny, who has punctured the anterior chamber for the relief of these affections, that this mass is always found in strings, flakes, or shreds; that the matter is of a tenacious character, and comes away in masses, usually in one mass, when it passes outward. It may be completely removed by seizing it with forceps, and drawing it out as you would a clot of blood or mass of lymph from any other situation." Dr. Reynolds' treatment in the case mentioned consisted in homatropine drops to be used every ten minutes until the pain ceased—this to be repeated at every recurrence of the pain, and five grain doses of the sulphate of quinine to be taken every hour during the day. Forty-eight hours after this change in treatment the cornea had cleared; in four days the entire mass of lymph effused into the anterior chamber had disappeared by solution and absorption. After this the quinine was administered three times a day before meals in the same manner, and dose (5 grains) up to the present time, ten days from the beginning of the trouble. The eye now being entirely free from irritation, and the general condition vastly improved, Dr. Reynolds further advised ten grains of the quinine every night at supper time for a week, as a precaution against relapse. Dr. Reynolds holds that cases of this kind, unless treated in the vigorous manner just described, go rapidly from bad to worse, until the cornea sloughs out *en masse*, and the eye is destroyed.

In reading the above case, one is inclined to suspect the quality, if not the quantity, of quinine used, but it is important, as an indication of the teaching and treatment with regard to these important cases in Philadelphia.

**Cocaine in Glaucoma.**—In the *Archives for Ophthalmology* for June last a case is reported by Dr. E. Sargent, of San Francisco, showing the value

of cocaine in glaucoma. The drug was instilled to relieve the distressing pain, which it did, but in addition there was noticed marked reduction in tension following its use. The case was one in which the right eye had 18 months previously been removed for absolute and painful glaucoma. In doing an iridectomy for relief of a subsequent glaucoma in left, the whole atrophic iris came away, and the eye became blind. When the patient, who was a woman of 70 years, came under the reporter's care, she was suffering severe pain, and the eye was of a stony hardness. She would not consent to enucleation, so a five per cent solution of cocaine, one drop every five minutes, until six drops had been instilled, was ordered. In twenty minutes the pain had been quite controlled, and as the pain was most apt to return in the evening and morning, the drops were repeated at these times for a day or two to ward off the pain. The interesting point in the case being that not only was the pain permanently relieved, but the tension was greatly reduced, so that the eye felt almost normal. No other treatment was employed, but it was noticed that the cocaine caused some dizziness and hallucinations, which gradually passed off, and this would seem to indicate that in other similar cases it would be well to try the effect of a weaker solution in the first place.

**A Case of Uncomplicated Glaucoma.**—In the same volume of the *Archives* our attention is drawn to a case of uncomplicated glaucoma occurring in a child of eleven years, noted by Dr. R. L. Randolph. The patient was a mulatto girl, and the dimness of vision had been noticed more than a year, so that the glaucomatous state must have been present in her ninth year. The intraocular changes were well marked and typical, but the interest of the case lies in the age of the patient, for simple uncomplicated glaucoma is very rare indeed in the young subject.

**Persistent Pupillary Membrane.**—In the *Medical and Surgical Reporter*, published in Philadelphia, for April 16, 1887, we have a case reported by Dr. C. S. Turnbull of Persistent Pupillary Membrane. The special interest, besides the rarity of these cases, lies in the fact that in this subject the persistent pupillary membrane, a remnant of fetal life, could be seen to consist of hair-like fibres attached to the anterior surface of the iris, and in no way attached to or interfering with the pupillary margin of the iris, the so-called constrictor muscle. So that this case may be taken as settling the disputed question as regards the origin of these fibres.

**Iridectomy in Relapsing Iritis.**—In the *Gazette Médicale de Paris* for 19th March there is a note on a thesis by M. E. Saint-Martin on this subject. The author gives due credit to Graefe for drawing attention to the great value of iridectomy in these cases. He points out, as the teaching derived from a number of cases in hospital, that, while the surgeon should prefer to wait until the more acute symptoms have passed, if synechia (posterior) have formed, it is advisable to operate; and certainly, if the communication between the anterior and posterior chamber is closed by the inflammation, it is best to do an iridectomy to avoid if possible glaucomatous conditions supervening.

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HER MAJESTY has been pleased, by and with the advice of her Privy Council, to nominate Professor Sir George H. B. Macleod, M.D., Surgeon-in-Ordinary to the Queen in Scotland, to be for five years a Member of the General Council of Medical Education and Registration of the United Kingdom for Scotland, in the place of the late Dr. Andrew Fergus.

*Books, Pamphlets, &c., Received.*

- The Throat and its Diseases. With 120 Illustrations in Colour and 200 Engravings, designed and executed by the author, Lennox Browne, F.R.C.S.E. Second Edition. London: Baillière, Tindall & Cox. 1887.
- Gout and its Relations to Diseases of the Liver and Kidneys. By Robson Roose, M.D. Fourth Edition. London: H. K. Lewis. 1887.
- Annual Report of the Board of Regents of the Smithsonian Institution, to July, 1885. Part I. Washington: Government Printing Office. 1886.
- Le Charbon des animaux et de l'homme. Par J. Straus. Avec 4 figures et une planche. Paris: Au Bureaux du Progrès Médical. 1887.
- Sprains: their Consequences and Treatment. By C. W. Mansell Moullin, M.A., M.D.Oxon. London: H. K. Lewis. 1887.
- The Principles of Antiseptic Methods applied to Obstetric Practice. By Dr. Paul Bar. Translated by Henry D. Fry, M.D. Philadelphia: P. Blakiston, Son & Co. 1887.
- A Dictionary of Terms used in Medicine and the Collateral Sciences. By the late Richard D. Hoblyn, M.A.Oxon. Eleventh Edition. Revised throughout. By John A. P. Price, B.A., M.D. London: Whittaker & Co. 1887.
- Lectures on Surgical Disorders of the Urinary Organs. By Reginald Harrison, F.R.C.S. Third Edition. London: J. & A. Churchill. 1887.
- A Text-book of Midwifery. By the late Otto Spiegelberg. Translated from the Second German Edition, by J. B. Hurry, M.A., M.D. Vol. I. London: The New Sydenham Society. 1887.
- A Handbook on Diseases of the Skin. By Robert Liveing, A.M., M.D. Fifth Edition. London: Longmans, Green & Co. 1887.
- The Rectum and Anus: their Diseases and Treatment. By Charles B. Ball, M.Ch. With 54 Illustrations and 4 Coloured Plates. London: Cassell & Co. 1887.
- A Treatise on the Animal Alkaloids, or the Ptomaines and Leucomaines. By A. M. Brown, M.D. London: Baillière, Tindall & Cox. 1887.
- Manual of Hypodermic Medication. By Drs. Bourneville and Bricon. Translated from the Second Edition, by Andrew S. Currie, M.D. London: H. K. Lewis. 1887.
- Vaccinia and Variola: a Study of their Life History. By John B. Buist, M.D. London: J. & A. Churchill. 1887.
- On the Operative Surgery of Malignant Disease. By Henry T. Butlin, F.R.C.S. London: J. & A. Churchill. 1887.

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ORIGINAL ARTICLES.

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EPIDEMIC PNEUMONIA.

By W. LOUDON STRAIN, M.B.(Glas.),  
Late Resident Assistant Glasgow Western Infirmary.

WHETHER pneumonia ever occurs as a widespread epidemic seems to be doubted, if not denied, in some quarters. That it is a rare occurrence is true, but that it does occur is also true, as will be evident from an account of such an epidemic which I am about to give.

For the sake of completeness it will be well to give a little idea of the locality in which the epidemic occurred. It is a small town of about 3,000 inhabitants situated in the centre of the world-famed Brazilian gold fields, about 3,000 feet above sea level, and 350 miles inland from Rio de Janeiro. With the single exception of typhoid fever, all other specific fevers are virtually unknown. Chest diseases are by no means rare, and phthisis is of late years becoming alarmingly prevalent.

The climate may be described as subtropical, and the seasons are divided into wet and dry. October to March are the wet months, the others being the dry. The sanitation of the place I will speak of in discussing the causation of the epidemic, the particulars of which I will now proceed to give.

The first case occurred in December, 1885, and the last in October, 1886.



By far the greater number of the cases were of an adynamic type, with a marked absence of excessively high temperatures and violent delirium; in fact, taking away the pathognomonic chest symptoms, and looking only at the temperature chart, and the objective symptoms presented, and resemblance to typhoid fever was more than striking. In the majority of cases there was no delirium at all, and, where present, it was of the typhoid type; I learned by experience to recognise the presence of this form of delirium as a bad omen.

Diarrhœa was frequently coexistent with the pneumonia, and formed one of the most troublesome complications and sequelæ; and it is interesting to note that, from the month of July, 1885, on into the wet season 1886-87, there was a great increase in the prevalence of diarrhœa in the district.

The mortality was exceptionally high, 6 out of the 24 cases treated in hospital proving fatal. Two died on the 7th day, 2 on the 10th, 1 on the 8th, and 1 on the 13th. The remaining one died on the 59th day from acute phthisis. Recurrence of the attack within six months occurred in 3 of the cases.

The following table shows the number of cases in each month. The numbers in brackets indicate the number treated in hospital, and it is from these chiefly that my inferences and conclusions are drawn, as they were more under my personal observation, and in the fatal cases diagnosis was verified by *post-mortem* examinations:—

TABLE I.

|       |           |           |       |       |            |          |       |
|-------|-----------|-----------|-------|-------|------------|----------|-------|
| 1885. | December, | 3 cases.  | (3)   | 1886. | June,      | 4 cases. |       |
| 1886. | January,  | 1 case.   | (1)   | „     | July,      | 19       | „ (6) |
| „     | February, | 2 cases.  |       | „     | August,    | 8        | „     |
| „     | March,    | 1 case.   | (1)   | „     | September, | 9        | „ (2) |
| „     | April,    | 13 cases. | (8)   | „     | October,   | 4        | „ (1) |
| „     | May,      | 11        | „ (2) | „     | November   | 0        | „     |

Compare with this the fact that for the year preceding December, 1885, only 1 case was treated in hospital, and for the year 1884, only 4 cases were so treated, and that from October, 1886, up to May, 1887, not a single case has been in hospital, and it is obvious that in 1886 there was an undue prevalence of pneumonia amounting to the proportion of an epidemic.

Some special cause must have existed to bring this about. *Ca va sans dire.* What was that special cause? The most notable thing to account for it was the remarkable falling off in the rainfall for the wet season 1885-86.

TABLE II.—RAINFALL FOR YEARS 1883-84 TO 1886-87.

|                         | 1883-84.       | 1884-85.   | 1885-86.   | 1886-87.   |
|-------------------------|----------------|------------|------------|------------|
| WET SEASON. DRY SEASON. | October, . . . | 4·36 ins.  | 8·25 ins.  | 9·50 ins.  |
|                         | November, . .  | 9·78 "     | 9·17 "     | 1·08 "     |
|                         | December, . .  | 13·12 "    | 13·17 "    | 7·82 "     |
|                         | January, . . . | 16·09 "    | 17·98 "    | 4·82 "     |
|                         | February, . .  | 18·14 "    | 16·53 "    | 9·71 "     |
|                         | March, . . .   | 5·12 "     | 7·79 "     | 6·63 "     |
|                         | April, . . .   | 2·08 "     | 0·00 "     | 1·86 "     |
|                         | May, . . .     | 1·22 "     | 0·03 "     | 0·00 "     |
|                         | June, . . .    | 0·00 "     | 0·00 "     | 0·00 "     |
|                         | July, . . .    | 0·00 "     | 0·00 "     | 0·00 "     |
|                         | August, . . .  | 0·07 "     | 0·00 "     | 3·06 "     |
|                         | September, . . | 0·48 "     | 1·89 "     | 3·13 "     |
| Totals, . . .           | 70·46 ins.     | 74·81 ins. | 47·61 ins. | 64·42 ins. |

A glance at this table shows the great deficiency in 1885-86, and it will also be noticed that the preceding dry season was an exceptionally dry one, barely 2 inches of rain having fallen in 6 months.

I have carefully gone over the records of the diurnal maximum and minimum temperatures, but find nothing exceptional in these years as compared with others.

In order to understand the connection between the rainfall and the epidemic, it is necessary to say a little about the sanitation. The habits of the natives are of the filthiest kind, and so far as they are concerned, absolutely no sanitary arrangements exist of even the crudest description. The beneficent influence of Heaven's three precious gifts—air, light, and water—is to them almost *nil*. No drainage is attempted, although the hilly character of the country is eminently favourable for it. The idea of a water closet, or indeed, of a common privy, seems never to have occurred to them, and consequently, all refuse and excrementitious matters find a lodging at their very doors, aye, and very often inside their doors. A little clump of brushwood, three feet high, will be used as a common privy by a whole street, provided it is *near enough*. The water for household use is all carried, in many cases, long distances, and so is reckoned rather a precious commodity. Such being the state of affairs, it is easy to understand that with three months of dry weather the ground in and surrounding their overcrowded houses becomes well saturated with all kinds of putrefying organic matter. Add to these, pigs, dogs, cats, fowls, and goats, which have the run of the house, and I think matters are about as bad as they can be.

As already stated, the country is hilly and so is favourable for natural as well as artificial drainage. The heavy deluges which we have, become, then, almost the sole agent in warding off consequences which would be disastrous to the inhabitants. What occurs during the heavy rains is, that all the superficial soil is washed clean away to the rivers, leaving a clean clayey or gravel surface behind. In the month of December last, 5 inches of rain fell in 24 hours, and the rainfall for the month was 24 inches, or within 6 inches of the yearly rainfall in many parts of Britain. After such a deluge as this, truly old things have passed away and all things have become new. But in the season 1885-86 those heavy deluges were conspicuous by their absence. The rain, for the most part, instead of carrying clean away the superficial layers of soil, served only to "puddle" them, and with a few days of hot sun, this again returned to a state of dust, to be blown about by every breath of wind, impregnating the atmosphere of the ill-ventilated dwellings. That the epidemic was produced by the breathing of an atmosphere impregnated with the poisonous products of organic decomposition, I have not a doubt.

In conclusion, I think it would be well, in all cases of pneumonia, to eliminate defective sanitation as a possible factor in the causation of the disease, just as is done in typhoid fever.

MORRO VELLIO, Brazil.

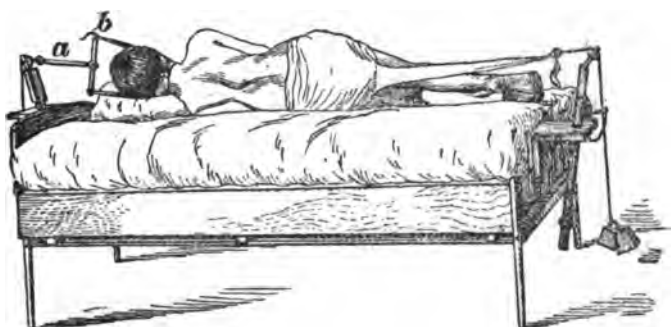
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## ON THE TREATMENT OF SPINAL DISEASE BY CONTINUOUS EXTENSION IN THE HORIZONTAL POSITION.

By WM. JAMES FLEMING, M.D.

SAYRE, in recent years, has familiarised all surgeons with the great benefit of extension, with more or less fixation, in diseases of the vertebral column. It is now very generally admitted that many cases are not suitable for any of the varieties of "jacket-treatment." Frequently, nothing short of the maintenance of the absolutely horizontal position will suit the case, and the improvement under this *régime* is slow, and only sometimes takes place at all. Besides, in many instances, notably with children, it is almost impracticable to carry the method out. I have had it in my mind for some years that, if we could add continuous extension

of the vertebral column to the horizontal position, great benefit would accrue. A case (No. I) presenting itself in November, 1886, I proceeded in a crude way to try the method, and was so much encouraged by the results that I have treated all suitable cases since then upon this principle, with, I consider, satisfactory success. The many excellent results obtained, both in my own hands and in those of others, by the application of the same general principle, by means of my inflating collar,\* pointed to the correctness of the method, and it only remained to elaborate the details of the appliance. I happened to mention my idea to Mr. Hilliard, the surgical instrument maker of this city, who informed me that he also had been at work in the same direction, and I have gladly availed myself of two of his suggestions—viz., the interposition of an elastic cord at the head and the method of extension



from the pelvis by a single band from the sacral region—ideas which, he informs me, he has patented.

The apparatus, as now employed, is very simple, and can be easily understood from the cut. It must be borne in mind that elastic tension, constantly exercised upon living tissue, has an effect altogether disproportionate to what one would primarily expect from the amount of actual force employed. So that very slight traction, but continuous, is what we aim at.

The arrangement I now adopt consists of a strong elastic cord (a) six inches to a foot in length, one end of which is made fast to the top of the bed, and the other to the centre of a piece of wood rather longer than the greatest diameter of the head (b). The head stall—made after the pattern of

\* *Glasgow Medical Journal*, May, 1884, p. 379.

the one used for suspension in Sayre's method, but of chamois or other soft leather, and rubber tubing in place of side straps—is connected to the ends of this wooden yoke. A broad towel is tightly pinned or sewn round the pelvis, and to this, in the middle line behind, is firmly attached the end of a piece of strong calico bandage. The other end of this is tied, at about the level of the feet, to the cord passing over the common extension pulley at the end of the bed, and a weight, proportioned to the body weight of the patient, put on. The bed is covered with a macintosh sheet freely dusted with powdered boracic acid, and the arrangement is complete. It will be seen that, while the whole spinal column is thus acted upon by the extensile force, the arms and legs are quite free, and the patient can turn round and lie on the back, face, or either side at will, without for a moment relaxing the extension. The object of the smooth vulcanised rubber sheet is to reduce the great friction between the body and the rough surface of the sheet, which it was found entailed a considerable increase of weight.

In the majority of cases in which pain has previously existed it is almost immediately relieved. The patient experiences a sense of comfort; sleep and general condition improve. In some cases—indeed, where attainable, probably in nearly all—the use of massage and electrical stimulation of muscles is advisable, and can be employed without even momentary interference with the extension. In many cases it is advisable, after the cessation of all symptoms under this treatment, to make the patient for some time wear a poroplastic or other jacket, or, in cases of disease of the upper part of the spine, my pneumatic collar or Hilliard's pelvic jurymast.

The following are very short notes of the cases treated in my wards by this method up to date:—

CASE I.—J. L., æt. 29. 30th November.—Disease of 7th, 8th, and 9th dorsal. Treated six years ago in Edinburgh Infirmary with plaster jacket. Able to work for four years. About a month ago, while walking on the street, felt as if the bones of the spine were "slipping off each other." Since then, quite helpless; unable to stand without support. Pulmonary condition far from satisfactory—a suspicion of phthisis. Was treated by extension; dismissed in six weeks apparently quite well, but advised to wear a jacket. He reported himself some months afterwards quite well, as far as spine was concerned. I have since heard of his death from phthisis.

CASE II.—W. W., æt. 22. 10th February.—Spinal disease of traumatic origin treated by extension. 6th April.—Plaster jacket put on, and dismissed. 5th July.—Reported himself quite well ; can walk five miles without fatigue.

CASE III.—A. E., æt. 19. 4th May.—Very marked lateral curvature, with pain ; seems rapidly increasing. 25th June.—Dismissed well. Back nearly straight.

CASE IV.—F. W., æt. 8. 31st December, 1886.—Pott's curvature, 6th, 7th, and 8th dorsal. Muscular paralysis. Loss of power over sphincters. Dismissed 25th February, 1887, in plaster jacket, much improved.

CASE V.—Mrs. C., æt. 21. 3rd December, 1886.—Acute atlanto-axial caries of three months' duration. Treated with actual cautery and extension. 27th May.—Been able to walk about for some time with pneumatic collar, which on this date was replaced by Hilliard's pelvic jurymast. 2nd August.—Reported herself very well, but still requires to wear Hilliard's pelvic jurymast.

CASE VI.—J. K., æt. 13. 27th May, 1887.—Well marked acute high cervical caries, 10 weeks' duration. 14th June.—Able to sit up in bed without holding head with hands. 2nd July.—Free of pain, but a distinct depression can be felt over atlanto-axial joint, and movements of head rather less extensive than normal. Dismissed wearing pneumatic collar. 26th August.—Reported herself very well but still wears collar.

CASE VII.—N. M'L., æt. 11. 26th July.—History of injury a year ago. Pott's curvature, 2nd lumbar. Can only walk with hands on knees. Much better, but still in ward.

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## ON THE RELATION OF THE AIR WE BREATHE TO OUR COMMON DISEASES.

By FRANCIS HENDERSON, M.D.

(Concluded from page 101.)

To the question, Do air germs or organisms normally enter our bodies with the inspired air ? the authorities do not give a unanimous reply. Many of the leading experimenters and investigators in bacteriology answer in the negative. Thus,

Pasteur has "established that the bodies of animals in full health are sealed against the introduction of the germs of microscopic organisms; the blood drawn with sufficient precaution from the veins or the arteries could be exposed to the contact of pure air without putrefaction and without the appearance of living, thread-like organisms of any kind whatever, mobile or immobile." \*

Watson Cheyne, in the early chapters of his work on *Antiseptic Surgery*, records the details of numerous experiments, in which healthy blood and portions of the tissues were removed from the bodies of living animals, with antiseptic precautions, and it was invariably found (when there was no flaw in the *technique* of the experiment) that no organisms made their appearance, even under favourable circumstances, as to heat and moisture, &c. Similar results have been obtained by other experimenters. Watson Cheyne also found that "if putrid matter be injected into the jugular vein of the animal a few minutes before death, all the tissues removed and preserved, in the manner described, undergo putrefaction."† The general conclusion which he draws from these experiments is, "that the blood and tissues of healthy living animals do not contain organisms or their spores, and have no inherent tendency to undergo fermentation."‡ In our judgment, these experiments do not warrant this conclusion. The experiments prove that when blood and tissues are so removed from the living body, no signs of life subsequently develop, but it does not certainly follow that, at the instant of removal, they did not contain many kinds of germs. Neither the blood nor the tissues lose all their vital force for some considerable time after separation from the body, and their remaining vitality may be adequate to destroy the germ life which may be present.

That freshly drawn blood really possesses reserve power of this nature has been shown by Sir Joseph Lister. He found that blood drawn from the jugular vein of an ox into sterilised vessels, with antiseptic precautions, and allowed to coagulate, can resist the putrefying agents present in a small quantity of diluted putrid blood, remaining free from putrefaction and from the development of organisms.§ The same, we believe,

\* *Louis Pasteur: His Life and Labours*. By his Son-in-law. Translation, page 184.

† *Antiseptic Surgery*. By W. Watson Cheyne, M.B., F.R.C.S., &c. Page 48.

‡ *Loc. cit.*, page 249.

§ See "Report on a Study of Certain Conditions of Infection." By W. Watson Cheyne. *Brit. Med. Journal*, 31st July, 1886.

holds true as regards the tissues. For example, we have frequently observed that pieces of skin taken from the healthy living body, without antiseptic precautions, freely exposed to atmospheric dust for a considerable time, and manipulated by fingers which certainly were not *aseptic*, nevertheless succeeded perfectly in skin-grafting; showing that they were able to overcome the vitality of the putrefactive or fermentative particles which must have been clinging to them in abundance.

Again, it has been shown by many experimenters, that when organisms are injected into the blood of a healthy animal, even in considerable quantity, they are destroyed. Watson Cheyne italicises this general statement, as confirmed by his own experiments, that "*the blood and tissues, when in a healthy state, have the power of themselves of destroying organisms.*" He found, however, that the quantity must be limited, otherwise organisms make their appearance in the tissues of the animal. "The reason," he says, "that they are found when large quantities of bacterial fluid are injected, seems to me to be that, along with the bacteria, their products are introduced, that these act . . . as poisons, and that thus the resisting power of the animal is diminished."\*

Now, to argue, as we have seen Watson Cheyne does, that because the injection of putrid fluid prior to the removal of samples of blood and tissues causes them to putrefy, which does not occur if no putrid fluid is injected, to argue that this is a sufficient proof that these latter samples do not contain any septic germs at the instant of removal, is surely fallacious. It ignores the principle just stated, and which appears fully established, that whether healthy blood and tissues can destroy the germs or organisms which reach them is determined by the quantity of the latter, their stage of development, and their accompaniments. Portions of blood and of tissue taken from a healthy living animal *may* be quite able to destroy germs or organisms in certain normal quantities and states, and yet be unable to overcome the additional germinal matter and new conditions, which a previous injection of putrid material into the general circulation conveys to them.

As we are not aware of any more important or pertinent experiments than those cited, our conclusion is, that there is no decisive *experimental* evidence against the view that air germs or organisms normally enter our bodies with inspired air.

\* *Antiseptic Surgery*, page 252.



Let us next consider what light is thrown upon the subject by the phenomena of disease.

In the case of abscess, some pathologists maintain that micro-organisms are concerned with their inception; but at all events it is agreed that they are to be found at a very early stage. In many forms of inflammation also, as well as in other complaints, organisms are stated to be met with. How are we to explain their appearance? Are common germs or organisms normally kept out by the pulmonary and the gastro-intestinal membranes, and is their appearance in the tissues due to failure of the protective power of these membranes? Or, do germs and organisms normally pass through the membranes and are then destroyed by the blood and tissues, and these have failed in their action? In point of fact, when organisms are present, as in an abscess, the blood and tissues have failed to destroy them. If it is the function of the pulmonary membrane to exclude them, *it* has also failed. It seems improbable that two flaws in the bodily machinery in different situations, and possibly of a very different nature, must occur before organisms can make their appearance in the tissues, especially as it is known that this happens when the departure from perfect health is very slight. Thus, in the case of a small abscess, the signs of general illness may be hardly recognisable, and any constitutional disturbance that is observed appears, from the clinical standpoint, to be subsequent to, and dependent on the local mischief. If germs normally enter with the inspired air, then the failure of the blood and tissues to destroy them, which has certainly occurred in such a case, is sufficient to account for the appearance of organisms; even a localised failure is sufficient. But if germs are normally kept out, then the pulmonary membrane has also failed in its function, and the question is, What is the nature or cause of this defect?

Most of the larger dust particles, organic and inorganic, which pass through the glottis with the inspired air, are doubtless caught on the sides of the air passages, and are thence removed by ciliary movements, probably assisted by what has been described as the "scavenger action" of the bronchial muscles. It is hardly credible, however, that all the minute organisms or germs are thus intercepted, particularly if we consider that the force of the current of air through the smallest branches of the bronchial tubes is such, that its escape into the larger spaces of the infundibula and

clusters of air cells, is the cause of the natural vesicular murmur.\*

It appears to us certain, that many of the air germs must in this way reach the air cells; but are they here arrested and prevented from passing into the blood? We know that they are exceedingly minute. Dr. Burdon Sanderson proved that when suspended in a fluid (where we may suppose they are somewhat larger than in air, from imbibing moisture) they can pass through all kinds of filters, except one of unglazed porcelain.† Professor Tyndall especially has demonstrated how indestructible many germs are, and that, like many seeds, they are often hard and dry, and require a considerable time to get thoroughly wetted. Germs in air are in a very different condition from those in water. "In water they are already wetted and ready, under the proper conditions, to pass rapidly into the finished organism. In air they are more or less dessicated, and require a period of preparation more or less long, to bring them up to the starting point of the water germs."‡

These considerations certainly favour the view that air germs (whatever may be the fate of the developed organism) normally traverse the pulmonary membrane, and pass alive into the blood. If they do not, it must be due to one of two causes. Either, that these hardy organised particles are killed by contact with, or in the act of passing through, a thin membrane; or, that they are too large to permeate its pores. The first alternative implies a relatively superior vitality of the pulmonary membrane over germ life to which we know no parallel; and further, it implies that this overwhelming vital force becomes impaired or suspended by a very slight departure from perfect health. The latter alternative requires that when germs do enter the blood, the natural permeability of the membrane increases, and that such a change is the result of a diminution of the general vital energy, so slight as to be often difficult to observe. It must be admitted that both alternatives are highly improbable.

The above argument may be objected to on the ground that, when organisms appear in the tissues, as in an abscess, it is not certain that their germs have entered the blood through the lungs, for they may have entered through the

\* See Dr. Coats' paper "On the Seat of Origin of the Vesicular Murmur." (Reports of the Path. and Clin. Society).—*Glasg. Med. Journal*, Nov., 1886.

† "Lectures on the Infective Processes." *Brit. Med. Journal*, 1878.

‡ *The Floating Matter of the Air*, p. 105.

gastro-intestinal membrane. The reply is, that if it is held that our bodies are normally sealed against the entrance of micro-organisms, it is just as difficult to understand how the protective function of the gastro-intestinal membrane is interrupted by slight disorders of health; and, moreover, although we cannot prove that *common* air germs enter the blood through the lungs, we know from the effects they produce that *infectious* germs do enter by this channel, and there is no evidence that their passage through the lungs is owing to their specific pathological qualities, or to any other differences.

Let us consider this aspect of the question more fully. When an individual is in close and prolonged attendance upon a case of infectious disease, say small-pox or typhus fever, and yet does not become affected, what happens? Do no infectious germs reach his blood, although they must enter the lungs? Or, are they constantly passing into the blood, but not finding a suitable soil, or in consequence of the vital energy of the blood being too strong for them, they cannot grow and multiply, and so no effects follow? If we accept the first of these views, we are forced to conclude that the physical conditions in perfect health are such that these minute germs cannot pass mechanically through the pulmonary membrane. What happens, then, when a person in health contracts an infectious complaint by inhaling its germs? The germs must enter the blood in virtue of their vital force. They must effect a settlement on some part of the air passages or air cells, there grow and multiply, and afterwards penetrate the tissues and reach the blood. If this were so, we would expect that in every such case signs and symptoms would exist, caused by the reaction of the tissues against the invasion; but this is exceptional. There are some, no doubt, who appear to regard the throat as the seat of invasion of the scarlet fever virus, but this is not the general view. On the contrary we find Sir Thomas Watson approving of what he describes as a common opinion—viz., that the throat and the skin are the principal *vents* for the poison.†

It is impossible to discuss the subject here, but we believe it will be generally admitted that, in the great majority of infectious diseases which are communicated through the air, there is no evidence of bronchial or pulmonary mischief during life, and no traces of a battlefield are generally discoverable by *post-mortem*. And even in those complaints in

† *Principles and Practice of Physic*, Fourth Edition, vol. ii, p. 895.

which symptoms of this kind are usually present, as in measles and influenza, they cannot be regarded as marking the paths of invasion. Thus, there are cases of influenza without any catarrhal affection of the respiratory tracts.

Now, as we find no good ground for the theory that infectious germs, when they excite disease by being inhaled, do so by forcing their way through the lungs in virtue of their specific power of penetrating the tissues, we have to fall back on the other view—viz., that infectious germs are always entering our bodies when we are breathing infected air, and whether we become affected or not is determined after their entrance. If we do not, it is because they do not find the soil suitable, or because they cannot overcome the vital resistance of the blood and tissues. This is the general opinion, and is certainly the most reasonable. There are many considerations which seem to require its acceptance. Among others, there is the observed fact that persons who are constantly breathing an atmosphere loaded with a particular infection, if they do not take the disease at first, seldom become affected by prolonged and continuous exposure (provided, of course, no additional predisposing causes come into play, such as over-fatigue and anxiety). It seems impossible to explain this observation unless it is admitted that the poisonous particles are constantly entering, and that the blood acquires an increased power of resisting or destroying them. When continuous exposure ceases, this power appears to be gradually lost and susceptibility returns.

In his chapter on dissection poisons,\* Sir James Paget states that perfect immunity from the worst influences of these poisons may be acquired by those who are day after day engaged in *post-mortem* examinations, even although they have escaped any recognisable illness from inoculation by a wound. The immunity may last some years; but when one gives up dissecting, it gradually, like vaccination, wears out. Paget refers to Dr. Symes Thompson's personal experiences, which illustrate this point. On the same principle is to be explained the behaviour of the system towards other poisons, such as opium, tobacco, and alcohol. Persons may become accustomed by habit to take large doses; but if the practice were stopped for a considerable time, and then resumed with the same doses, poisonous symptoms would be developed.

There are other instances which go to strengthen the opinion that infectious germs do pass into the blood even when they do not produce their specific pathogenic effects.

\* *Clinical Lectures and Essays* (1875), p. 327.

Take the case of a malarious district. A certain proportion of the inhabitants are affected by intermittent fever or other marked form of malarial disease; but close observation leads to the conclusion that a great many, if not all, of the population suffer from some phase of cachexia or lowered vitality, which, through a series of graduated cases, is seen to be the result of the same poison which causes the developed complaint in the few. Again, when we study the histories of the great epidemics of influenza, such as those of 1833-37, we meet with the well attested observation that, during their prevalence, the vital tone of the whole community was in a depressed condition; and if acute inflammation occurred, it was observed to be of a remarkably asthenic type. Here we have evidence that the poison in the air entered the system of each individual, although in only a certain number did it prove capable of exciting the fully formed complaint. It is true that in neither of these diseases has the nature of the aerial poison been scientifically demonstrated; but that their etiology and phenomena can only be explained by the germ theory will be generally admitted. In the case of malaria the question appears to be now narrowed down to this—Is the *materies morbi* a bacillus, a plasmodium, or what other form of parasitic life? \*

Now, if it is the case, as is generally believed, and as these illustrations seem to teach, that infectious germs enter the blood through the lungs when they are present in the inspired air, *whether they take effect or not*, the inference is, that their passage is not due to their specific or pathogenic power. It is mechanical, and if so, we are not aware of any reason, such as a difference in size of particles, why common air germs should be excluded. In support of this opinion that germs enter mechanically, by which we mean in consequence of their smallness, we may refer to the evidence that inorganic and organic particles which have no vital penetrating power, can pass through the pulmonary surfaces. We shall only recall the fact, that lead-poisoning may result from inhaling the fine dust of the insoluble carbonate, in *white-lead* manufactories; and without further discussing this aspect of the question, we would refer our readers to Professor Oertel's work, where the subject is treated of in connection with respiratory therapeutics. †

This short survey of, what appears to us, the principal

\* See "An Address on the Hæmatozoa of Malaria." By William Osler, M.D., F.R.C.P.—*Brit. Med. Journal*, vol. i, 1887, page 556.

† Ziemssen's *Handbook of General Therapeutics*, vol. iii.

facts and considerations bearing on the subject, points to the conclusion, that common air-germs do normally pass into our blood with the inspired air. If, therefore, micro-organisms make their appearance, it is the blood itself or the tissues in which they are found which has failed to destroy them. Writing recently on the present position of the question, Dr. Burdon Sanderson says, "Micro-organisms certainly enter the human body normally by the physiological processes, but," he adds, "under normal conditions they are so rapidly destroyed that so far as their action is concerned they may be considered as non-existent. Under conditions which involve only slight departures from health, they may escape, but I doubt whether the ordinary microphytes of sepsis ever of themselves produce disease, *i. e.*, without the co-operation of other pathological agencies. Specific pathogenic microphytes *must* have as a *sine qua non* for their disease-producing power the qualities of being able to resist the destructive processes to which ordinary microphytes yield." We are not aware of Dr. Sanderson's reasons for his opinion that the micro-organisms which normally enter are so rapidly destroyed that so far as their action is concerned they may be considered as non-existent, and therefore, we cannot judge of the conclusiveness of his reasons; but on the hypothesis that a considerable part of the organised matter which normally enters the blood is in the germ state, and bearing in mind that air-germs are not easily killed, that those which are hard and dry require, according to Professor Tyndall, a considerable time to become thoroughly moistened, so as to reach the condition from which they grow into organisms, and having regard also to the wonderful potential energy which germs possess, remembering all this, it does not appear to us incredible that these particles which enter the body with the physiological processes may serve physiological purposes. This theory receives support from comparative physiology. Dr. Sanderson says, "In rabbits I have no doubt that organisms always exist in the tissues. In fish of all kinds they are abundant, and are prone to multiply whenever fish are kept in water of relatively high temperature." These facts are of much interest. If micro-organisms are always present in fish, they must be regarded as normally present, and if so, the analogy of Nature requires that they perform certain functions in the economy of the animal.

As regards the quantity of organised particles in ordinary air, Dr. Percy Frankland, who has made recent investigations, finds very great variations:—"Thus, the air of an extensive

heath near Norwich was found to contain from 5 to 7 micro-organisms in two gallons of air, whilst in that of a garden near Norwich were found as many as 31." Again, the number of micro-organisms falling on 1 square foot in 1 minute was found on one occasion, in a crowded railway carriage, to amount to 3·120. In a barn, in which flail-thrashing was going on, upwards of 8,000 organisms were falling on 1 square foot in 1 minute.\*

Dr. Angus Smith's investigations on this subject point to a larger quantity of organisms being inhaled. His method of "air-washing" by mechanically catching all sorts and conditions of the floating matter of the air, seems to the uninitiated, more inclusive, and therefore more suited for the mere purpose of enumeration than those methods, such as Hesse's, which go on the principle of offering a prescribed diet, such as Koch's jelly, and counting those organisms only which find it sufficiently suitable to their requirements as to grow thereon.†

Of Dr. Angus Smith's method the following is an example:‡ —Eighty-eight cubic feet of air were washed with a certain quantity of chemically pure, germ-free water. The liquid was allowed to stand until the germs it contained had time to develop into visible organisms. A sample drop was then placed under the microscope and the organisms counted, and the total number present in the whole quantity of air were thus estimated. These reached the startling amount of 37½ millions in the 88 cubic feet, which is about the quantity of air inhaled by an ordinary sized man in ten hours. The air which gave these results was Manchester open-air, collected at the back of the laboratory. "I did not observe any dust blowing," Dr. Angus Smith characteristically remarks, "but if there were dust, it was such as we may be called on to breathe."§

These calculations and experiments give some idea of the

\* "The Creatures we Breathe." *The Nineteenth Century*, August, 1887.

† "Messrs. Carnelley, Haldane, and Anderson, who used Koch's jelly in their experiments, state that their numerical results are not comparable with those of Miquel's, as his standard nutrient medium is different, and he employs a higher temperature."—*Loc. cit.*, page 65.

‡ *Air and Rain*. Dr. R. Angus Smith. Page 487, *et seq.*

§ The following figures, taken from the microbic statistics of Miquel, exhibit the relative purity of different atmospheres:—Ocean air, 0·6 bacteria per cubic metre; mountain heights, 1; the saloons of vessels, 60; the top of the Pantheon, 200; Park Montsouris (the average of five years), 480; at Berne, 580; Rue de Rivoli, Paris, 3,480; new houses at Paris, 4,500; old houses, 36,000; the new Hôtel Dieu, 40,000; the Hôpital de la Pitié, 79,000.—*Brit. Med. Journal*, page 894, 1885.

amount of germinal matter which may enter the lungs in a given time, but there are no data for estimating how much of this finds its way into the blood. But even supposing that only a small proportion passes into the system, it appears by no means unreasonable, that these minute particles, insignificant in substance but great in potentiality, may normally exert some influence on the blood, and may even stimulate certain changes in the tissues. On this subject Dr. Angus Smith says:—"If carbonic acid is not formed in sugar and albumen during the acts of fermentation and putrefaction without the aid of microscopic organisms, how is it that it is formed in the living being without the aid of such organisms? Or, do the organisms not increase its formation, to say the least?"\* But whatever may be the nature of the action of micro-organisms, if this is regular and constant, we must suppose that it serves some useful purpose. If so, we may fairly infer that, like other such agencies, a deficiency or an excess of micro-organisms may become causes of disease. We have seen evidence that an excess, such as exists in the air of dirty and overcrowded houses, is a factor in the unhealthiness of these dwellings. As regards the effects of a deficiency of organisms in the inspired air, we would ask attention to some observations which, if confirmed, are calculated to throw light on this point. Surgeon-Major Macnamara states:—"I have observed that, in places such as Suakin, where there is very little vegetation, men get scurvy occasionally, though they are supplied with vegetables. . . . Many a man gets on very well without eating vegetables, when he is surrounded by vegetation, but gets scurvy, under similar circumstances, in the desert or at sea,"†

It is a striking circumstance that the situations in which there is a deficiency or an absence of micro-organisms in the air, are just those in which scurvy is most liable to be developed—viz., in prolonged sea voyages, Polar expeditions, and in the desert.

In such localities we have testimony that neither fresh vegetables nor lime juice‡ can prevent its outbreak, though it may postpone and mitigate it. "It is not this or that particular article of food which occasions scurvy, but the absence of food which may be considered as belonging to a natural

\* *Air and Rain*, page 545.

† *Brit. Med. Journal*, vol. i, p. 491, 1886.

‡ Among Arctic explorers "the general opinion is that lime juice is the best, but by no means a certain, preventative." See *Refutation of the Report of the Scurvy Committee*. By Clement R. Markham, C.B., F.R.S.



diet."\* Here there is a basis for the speculation that it is the want of the living vegetable particles normally present in the air, and therefore part of *the natural diet of the blood*, which may be directly concerned in the causation of this disease.

Reflections regarding the possible effects of the life of the air upon the well-being of the body, effects which may vary not only with the quantity, but also, it may be, with the kind of organisms breathed, invest with deeper significance the physiological conclusion which was quoted at the beginning of this essay—viz., *all the tissues breathe in the blood*. While partaking of the oxygen which the blood carries to them, are the tissues not at the same time influenced by those other air-constituents which have entered the blood during its passage through the lungs?

### SUMMARY.

In the first part of the article we considered the extent and nature of the relationship between the air we breathe and our common diseases. Taking as the basis of our argument *The Vital Statistics of Glasgow*, we found it was there proved that house-accommodation, as a measure of air-space, is the chief factor in determining the death-rate. Confining attention to the common non-infectious diseases—namely, to those which are classified by name in the tables, we found it proved that the death-rate from these diseases is in direct ratio to the quality of the air—that as the air becomes impure from overcrowding and its concomitants, so does the death-rate rise, and *vice versa*. The inference is, that the prevalence of these diseases is largely regulated by the amount of certain air ingredients which are therefore *in some sense* the material causes of these diseases. It was further shown, that this broad conclusion was, in the case of Glasgow, not vitiated by the possibility of the material causes of diseases being conveyed into the system by the water supply, because that being uniform throughout the whole city, it could have no share in producing the different death-rates in the different districts. Again, it was argued, that neither did *predisposition* greatly affect this conclusion, because although its existence might be necessary to render these air ingredients capable of exciting disease, still, in so far as *predisposition* is not itself caused by the action of material causes, it must be regarded, in dealing with such large numbers, as affecting

\* Dr. Pavy's evidence before the Scurvy Committee. *Loc. cit.*

in a nearly equal degree, all the inhabitants of the compared districts of the city. \*

Lastly, it was pointed out that (with the exception of the acute pulmonary diseases which in Glasgow are under special influences), all our common diseases decreased in equal proportion by the addition of purer air—that even in the best districts of Glasgow they maintained the same relative frequency to each other—that they were met with in the best country air, and that they reached a minimum of frequency in *extra-pure* air, pointing to the conclusion, that certain ingredients present in all ordinary air, which may minister to the well-being of the body in health, may become, in altered conditions, the material causes of our common diseases. Putting aside this last inference as raising questions requiring further elucidation, there remains the very important and comprehensive conclusion which we hold to be fully established, and which we desire to be fully weighed—viz., that the prevalence of our common diseases *cæteris paribus*, is in direct ratio to the amount of certain ingredients in the inspired air.

In the second part of this article we discussed the question, What are the constituents in the air which are chiefly concerned in the relationship between the air we breathe and our common diseases? The conclusions arrived at were—first, That it is the organic matter of the air which supplies the connecting link ; second, That of the two kinds of organic matter, there can be little doubt that the living air particles are the essential material causes, although the dead organic products, when present in considerable amount, in all probability, by depressing the vital powers, promote the action of the organisms. The principal reasons for the conclusion that the living air particles are essentially concerned in the production of these diseases are (1) That in some (at least two) of our common diseases the material causes have been isolated, and have been proved to be micro-organisms ; (2) that in none of the common diseases has the material cause been demonstrated to be other than an organism ; (3) that in nearly all, if not all, of the common diseases, organisms have already been found in the tissues. It must be admitted, of course, that their appearance is consequent upon some

\* We may be underrating the influence of *predisposition* in causing the higher death-rate of insanitary districts, but it must not be overlooked that in these districts the bad effects of *hereditary* predisposition are much lessened by the great mortality of children under five years of age, whereby a large proportion of the constitutionally weak are carried off, and the comparatively robust remain to propagate the race.

antecedent disorder or derangement, otherwise there would be no such thing as health; still, whenever they grow, they become at once the *causes* of symptoms and of pathological changes, and it is known that they (the organisms) appear at a very early stage in some of these diseases.

With regard to the dead organic matter, if present to a large amount, as in the air of overcrowded houses, it may be supposed capable of having a poisonous influence. If so, we would expect its action to resemble that of an organic poison, such as opium or strychnine, if they were diffused through the air—that is to say, it would (allowing for idiosyncrasies) produce the same symptoms in all the persons exposed, according to the amount of poison inhaled and the age of the individual. If purer air were added to this poisoned atmosphere, the symptoms would diminish, and if further additions were made they would gradually cease. But this is not in the least a correct description of what is observed to be the effects of the impure air of overcrowded dwellings. Such an atmosphere invariably produces an abundant crop of all our common diseases—that is to say, it gives origin to a number of distinctly separate disease-entities, each of which is well known. The uniform production of such perfectly distinct and definite effects, necessarily implies the existence of differentiated causes. Now, consider what happens when a supply of purer air is added to this impure atmosphere. The number of cases is diminished, but the several diseases remain unchanged in character and unchanged in their relative frequency. These phenomena are easily intelligible, if the material causes of disease present in the air are *particulate*, and if each particle after its kind possesses individual and specific powers, but they are not explicable, as far as we can see, upon any other hypothesis. The addition of purer air does not alter the quality of the particles, but it lessens their number in a given measure of air, and their power depends on number. This is just what is observed in regard to the infectious diseases, the material causes of which are known or admitted to be, organised particles. Dilution with air or fluids (water or milk) limits their disease-producing power to the few—namely, to those most susceptible. The same principle appears to hold true in the behaviour of some common organisms. Thus, Watson Cheyne has found that the effects of the injection of the micrococci, which can cause suppuration, are greatly dependent upon the dose.\*

\* "Report on a Study of Certain of the Conditions of Infection. *British Medical Journal*. 31st July, 1886.

Now, how does the proposition, that the unhealthiness of overcrowded houses is associated with the number of organisms, compare with the results of experimental investigations into the quality of the air of such places? It is consistent with these as far as they go. Prof. Carnelley, Mr. Haldane, and Dr. Anderson, in their research found that as they passed from 4 roomed to 3, 2, and 1 roomed houses, there was an increase in the organic matter, and *more especially* of the micro-organisms, and a corresponding increase in the death-rate, together with a marked lowering of the mean age at death. Further, their experiments led to the conclusion that the amount of organic matter was chiefly due to influences contaminating the room at the time of observation. On the other hand, experiments showed that micro-organisms in the air of a room do not come, to any large extent, from the persons present at the time, and therefore they must come from the room itself. "Clean" houses were found to contain few organisms; "dirty" houses, 3 to 5 times as many. Now, it is quite certain that the "clean" are the healthy and the "dirty" the unhealthy houses. If it may be assumed that *new houses*, taken in the mass, are comparatively healthy, and *old houses* comparatively unhealthy, then Miquel's microbic statistics exhibit micro-organisms and disease as close companions. His averages show that microbes in old houses in Paris are about eight times as numerous as in new houses.

This is the drift of the present experimental evidence, connecting prevalence of disease and numbers of micro-organisms.

As to the organic matter, the experiments hitherto made do not more exactly define its position. Professor Carnelley, Mr. Haldane, and Dr. Anderson, consider that it has a very great effect in lowering the general health and predisposing to other diseases, but they do not find that a positive conclusion as to the influence of the separate constituents of contaminated air can as yet be arrived at from experiments.

In the absence, therefore, of experimental facts, the place and share of the living particles, and of the dead organic matter of the air must, for the present, be judged of on more general grounds. We have stated above what appears to us the chief reasons for the view that the material causes of our common diseases are the common organisms. These, however, we know are not capable of growing in the tissues, unless the healthy vital resistance of the latter, is in some way interfered with. Now, as the dead organic matter when in considerable quantity, possesses the power of depressing vitality, it is

reasonable to infer that its action will promote, and may even largely determine the growth of organisms. Thus, it appears that both organisms and organic matter are concerned in the production of disease in unhealthy houses. We do not know which of the two is the most important factor in the ordinary combinations of dirt and overcrowding; but the share contributed by each must vary. It will depend upon the relative amount of each operative in the particular case, because we know that the power of organisms, as well as of the organic matter, is a question of quantity. There is, however, this great difference. The organisms are essential to the development of the disease-process, whereas, the organic matter is not essential; for our common diseases occur in circumstances where it is impossible to suppose that the organic matter in the air can be present in effective quantity. Whatever disorders the proper working of the body, by disturbing the circulation, arresting the equable flow of nerve energy, altering the natural chemical condition of the tissues, &c., results which may follow from the action of cold, from meteorological changes, from improper or insufficient food, from exhaustion of body, or anxiety of mind, &c., may act *without* as well as with the depressing poison of organic matter inhaled, in determining the onset of disease, by enabling the organisms to grow and multiply in the tissues.

But, again, it is clear that the opinion we form of the etiological significance of organisms, must be greatly influenced by the answer to the question which has been discussed in the previous pages—Do common germs normally enter our bodies? If they do, which is the opinion we consider the balance of evidence favours, then it follows that in unhealthy houses a constant excess of these potent particles must pervade the system, and the risk of their growing and multiplying by the aid of comparatively weak determining causes must be intensified. These conditions will naturally tell most upon young children and delicate persons.

But, even if the other view be correct, that germs do *not* enter the body in health, it is proved that in consequence of slight departures from health, they enter and grow, for they become visible in the tissues. Thus, in any case, organisms *may* grow at very early stages, and in point of fact this is known to occur in some diseases. Now, the appearance of organisms implies that there has been a struggle between the life of the tissues and the living invaders, in which the latter have been victorious, and, therefore, their antagonistic influence upon tissue life must considerably precede the

period when they become recognisable in the tissues. These considerations indicate that the influence of organisms may come into play at a very early stage in the genesis of our common diseases.

There are many other questions bearing upon this important subject, but we fear that we have already more than exhausted the patience of our readers. The important conclusion, abundantly established by statistics, that the prevalence of our common diseases is largely dependent on the quality of the air, proves the *fact* of relationship; and we have attempted in this essay to show how the nature of the relationship may be explained in the light of recent knowledge regarding the constituents of the air and their several actions upon the body. We are well aware that much that has been brought forward is theoretical, and that many of the conclusions can only be regarded as provisional; but the article will have served some useful purpose, if it exhibits the points where facts are most needed to strengthen or to correct the argument, and if it induces others to examine the subject for themselves.

Etiology is a difficult and obscure study, and it cannot be wholly dissociated from theories and mere opinions; but it should not on that account be avoided. We can scarcely hope ever to understand fully the causation of disease; but the more attention the subject receives, and the more we practise the art of analysing complex and varied causal influences, the greater will be our success in estimating aright the share contributed by each of those factors we learn to recognise, and the more surely shall we lay the foundations of the scientific treatment of disease.

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## LECTURES ON THE DISEASES CLASSIFIED AS TABES MESENTERICA.

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(Continued from page 23).

### CASES IN ILLUSTRATION, WITH REMARKS.

THE four cases which follow were published in the *Medical Times and Gazette* of August, September, and October, 1885, as following a series of observations on acute peritonitis in the preceding year; and it was intended to have followed

up the subject in succeeding papers, to somewhat the same effect as in the preceding lectures; only preserving more than has been done in them the form of clinical instruction addressed to students of medicine. The *Medical Times and Gazette*, however, was brought to an abrupt conclusion at the close of 1885, and hence it became necessary to reprint these cases in their present connection, as a part of the evidence of what has been submitted in a more doctrinal form. Each case, from this point of view, may be regarded as a type from which a definite lesson can be drawn, And it has been considered better to present a few facts, very carefully observed, than to multiply instances. With the exception of Case IV, none of the others can be said to be nearly unique in the experience of the author.

CASE I. (*Medical Times and Gazette*, 29th August, 1885, page 273.)—*Peritoneal disease with fluid effusion, in a child; evidences, in all probability, of chronic peritonitis, and, at a later period, of thickening of the great omentum. Symptoms exceedingly mild, with entire absence of organic complications, and little, if any, fever. Cure apparently complete.*

Jane M., æt. 8, was admitted to the Western Infirmary on 10th April, 1885, and dismissed, apparently well, on 14th July. During this three months' residence she was repeatedly and carefully examined by a considerable number of persons, and owing to the clinical examinations for the degree being in progress at the time, she was repeatedly employed as a typical case—*i. e.*, as illustrating the physical signs of fluid in the abdominal cavity, with certain peculiarities of local distribution. The description in the *Medical Times and Gazette* gives, in more detail than is perhaps necessary here, the progress of the case and the precautions employed in the diagnosis. It may suffice to record at present that the child was stated to have been "always somewhat swollen in the belly;" but that towards the end of March an increase of tension, with some pain, caused a degree of alarm. The bowels had been regular, except during an attack of measles at the beginning of the year, when they tended to become loose. The appetite was much diminished, but there was no vomiting. A slightly rickety condition, soon passing away, at two years of age, and previously to this whooping-cough, seemed to comprise the entire morbid history of any consequence; and it may be mentioned that the circumstance of an elder sister

suffering from what was called "consumption of the bowels," had led her parents to look very carefully for the slightest traces of any similar complaint in this girl.

Within a fortnight from her admission on 10th April, it was ascertained that the circumference of the abdomen, evidently too great from the first, had increased from 25 to 28½ inches, and the signs of fluid accumulation, at first not evident, became such as to leave no doubt whatever, and even to give the characters of what might have been a well marked ascites, were it not that during its whole progress the *levitation* of the intestines in the umbilical region, was markedly interfered with. "The inference I drew from the whole facts at the time was that this fluid effusion was not a mere ascites, but some more or less fibrinous exudation which hampered the free movements of the intestinal coils among the fluid, and prevented them from following the physical law which in moderate ascites, or serous dropsy of the peritoneal sac, usually causes them to float up towards the higher levels, according to the position of the body at the time." Palpation also gave slight indications of increased resistance in the umbilical, as compared with the sub-umbilical, regions of the abdomen. The abdominal superficial veins, too, were decidedly increased in volume at this date.

Notwithstanding these somewhat ominous physical signs, however, the general condition of this little patient was extremely favourable. There was no acute suffering of any kind. The utmost that could be said was that the child was rather indisposed for play and for active movements, though quite able to sit up without fatigue for several hours each day. The temperatures indicated only slight febricula (max., 100·4° F.) The period most characterised by these slightly abnormal temperatures was from 19th to 26th April; both before and after this period the differences between the morning and evening temperatures were insignificant; and after the 1st May no abnormal temperatures at all were noted.

An increase in the body weight of 3½ lbs., concurring with the physical signs above referred to, was discounted as probably due entirely to the fluid accumulation, possibly even concealing a real loss of tissue weight. The urine, relatively scanty during the period of active effusion, increased from a daily average of 17 ounces to 24 ounces after the close of this period. The lungs and all the organs, so far as they could be investigated with the utmost care



throughout the illness, presented no signs of disease. The bowels were regular, and the tongue absolutely clean.

Four days before she was dismissed from the hospital, apparently quite well, a most deliberate and minute survey of her condition, for purposes of clinical instruction, was entered in the journals, and formed the basis of remarks to be found at length in the *Medical Times and Gazette*. The practical result of the whole was that almost every trace of fluid effusion had disappeared, the circumference of the abdomen declining to a certain extent as compared with the maximum above-mentioned, but being, on the other hand, manifestly reinforced by the considerable increase of muscular flesh and fat in the abdominal wall, "so that this child is now only  $\frac{7}{8}$ th of an inch less in girth than when admitted on the 10th of April with an obviously enlarged abdomen." In other words, with a circumference of 25 inches on admission, the child was quite manifestly too large and distended as regards the abdomen; four days before her dismissal, with a girth of  $24\frac{1}{8}$ th inches, she was not distended at all, the abdomen being almost, if not quite, in due proportion to the rest of the body. Between these periods the girth, as stated above, had been  $28\frac{1}{2}$  inches.

The point of greatest interest, however, in this final examination, was the evidence obtained, on very critical palpation and percussion of the abdomen, tending to show that an appreciable, though slight, amount of thickening of the great omentum still persisted after the disappearance of the fluid, and after the return of the child to apparently good health. The complete details of this evidence will be found in the *Medical Times and Gazette*. It was considered as not only demanding careful exposition as a clinical lesson in itself, but as entirely corroborating the previous impressions derived from the state of the physical diagnosis during the period of effusion. Taken in connection with the gain in weight ( $2\frac{3}{4}$  lbs. since the absorption of the fluid ceased to be a counterpoise to the putting on of flesh), the obviously improved general condition, and the absence of all symptoms of disease, it was not considered to be a state of matters interfering with the reasonable hope of a practical recovery.

This hope was fully justified; for, nearly two years afterwards, I heard of this patient as still practically quite well.

The treatment was exceedingly simple; it consisted of inunction with cod liver oil, the use of extr. hordei, pancreatic emulsion, and careful regulation of the diet. Cold compresses were employed up to 25th April, but were then

found to be unnecessary, and the oleaginous application was substituted. Gastric tonics, &c., did not appear to be required at any time.

In reporting this case for publication in a medical journal, my object was that it should, as far as possible, tell its own story, independently of technical nomenclature. But the reader of the preceding pages will have no difficulty, I think, of bringing the case into relation with the general subject of what has been called *tabes mesenterica* or *carreau*. As regards the question of its tubercular origin, the evidence is (happily) wanting. All that can be said is, that in a sister of this little girl the symptoms of a disease in some respects similar had actually been interpreted as those of "consumption of the bowels." The doubt which attaches necessarily to the pathology of the cases which get well, as remarked in the previous chapters of this memoir, hangs over this case to the full extent. Not so with the following case, in which there was a fatal result, but no *post-mortem* examination.

CASE II. (*Med. Times and Gazette*, 19th September, 1885, p. 389).—*Typical case of tubercular peritonitis, with coincident disease of the intestinal mucous membrane, and probably also of the mesenteric glands. Gradual emaciation, with many fluctuations, and ultimately death, with all the features of tabes mesenterica, as described.*—Mary Jane S., æt. 10, admitted to the Western Infirmary 13th March, 1885. When this girl was first seen, it was felt to be impossible to handle or disturb her much. Her condition resembled a good deal that of a case of typhoid fever, with marked abdominal complications; it was, in fact, not quite certain that this was not so, although there was no characteristic eruption. There was great tenderness of the whole abdomen, which was much distended, and the superficial veins were very much enlarged. Had it been a case of typhoid fever, I should have been apprehensive of perforation, either actually present or impending. The face was flushed and bedewed with perspiration. The temperature, however, did not exceed  $101.6^{\circ}$ , and I noticed that she had not the peculiar aspect of acute peritonitis from perforation; nor had she the disposition to draw up the knees and to lie in a constrained position on account of the pain. The physical signs at this time were, (so far as they could be gone into safely) very much the same as

those in Case I at a somewhat advanced stage of its progress, and this although the whole of the symptomatic phenomena and the urgency were so different in the two cases. There were signs, also, of a mild general bronchitis, but without any expectoration. The percussion of the chest was normal.

The history was, briefly, as follows:—She had passed for a healthy child up to three weeks before admission. Then began a series of symptoms closely corresponding with those of *tabes mesenterica*, or “consumption of the bowels;” flatulence, with pain, tumid abdomen, diarrhoea, some cough, emaciation, and probably more or less of fever. It was the diarrhoea, gradually increasing in severity, which finally drove her away from school, which she had been attending up to a week before admission. Vomiting had been an early, but by no means a persistent symptom, being brought on, apparently, by some powders prescribed for her. There was nothing to lead to the suspicion of any serious illness previous to the present.

The treatment at once adopted, even before all these particulars were fully ascertained, was by opium in  $\frac{1}{2}$  gr. doses, with gr. ij of quinine three times a day. Fomentations were applied to the abdomen, evidently with much relief. On the third day it was possible to make a more detailed examination of the abdomen, and although it was still necessary to use great caution, I was able to demonstrate to several members of the class facts which were at the time roughly indicated in a diagram; but, as the attempt to reproduce this by a woodcut in the *Medical Times and Gazette* was not very satisfactory, it is omitted in this reprint. Generally speaking, the right side of the abdomen and the umbilical region were almost everywhere more or less dull to percussion, the left side much less so. The gastric region, and some of that of the transverse colon, were normally clear; and the left iliac and hypogastric regions were at least not notably dull. It was also observed that the dulness was throughout *easily penetrated*—i. e., by employing a slightly increased strength of stroke; and this relative dulness on percussion corresponded well with the tactile examination in giving the signs of thickening of the parts overlying the intestines, without fluid effusion; and, in particular, thickening at least on the right side of the parts anatomically related to the great omentum. The details of this examination, as it was recorded for clinical purposes on the 16th of March (three days after admission), are commented on at length in the *Medical Times and Gazette*, although, as already

stated, the diagram in illustration is so unsatisfactorily brought out as to be of no use to the reader.

The remedies employed from the first very greatly relieved the pain, and also completely controlled the diarrhoea, so much so that two days after this, constipation (evidently caused by the opium) was the predominating condition. The abdominal tension was much reduced; the temperature became normal; pulse 96; tongue nearly clean and natural; so that in four days this girl seemed to have passed from a prospect of immediate death to a state of almost complete convalescence as regards the acute symptoms. Even the details of the physical examination underwent a corresponding modification; and after a short time it was found possible to intermit the opium and quinine, and to substitute very gentle friction of the whole abdomen with cocoa-nut oil, for the fomentations. Under this treatment, for some weeks at least, the progress appeared to be that of an uninterrupted convalescence. In the course of the month of April, however, the improvement came to be regarded as more doubtful, although there was no return of any of the more obviously unfavourable symptoms. On 6th May, she had been losing rather than gaining in weight; the temperatures, which had continued normal up to 16th April, were again more or less disturbed, and in ten days in succession showed a mean of  $98.5^{\circ}$  morning, and  $100.4^{\circ}$  evening, with a maximum of  $102^{\circ}$ . But a still more patent fact in the direction of an unfavourable change was the existence of manifest signs of a considerable amount of gravitating fluid in the abdomen which, concurring with the above symptoms, certainly appeared to indicate a new period of sub-acute morbid change. During the entire months of May, June, and July, there was a progressive loss of body weight which, although not more than 6 lbs. in all, was not fully accounted for by the absorption of the fluid effusion. All the facts now corresponded with a downward progress, though perhaps not a rapid one; the increasing languor, the occasional pain, the dispirited look and manner, were very striking; although the appetite was well maintained and there was no diarrhoea. At a still more advanced period (in September), although there was no pain at all, and almost no active symptoms of any kind, there was slight febricula (maximum  $101.2^{\circ}$ ) and in this state the case was reported as one probably of tubercular disease, more or less widely distributed in the abdomen, perhaps also in the thorax, so as to include the mesenteric glands

with the peritoneum and the mucous membrane. This character the case retained for a month or two longer, when the death of this child was reported to me as having occurred in the East Park Home, without any essentially new symptoms. No opportunity occurred of completing the knowledge of the facts by a *post-mortem* examination. The treatment latterly need hardly be discussed, as it was of little use.

CASE III. (*Med. Times and Gazette*, 26th September, 1885, page 423.)—*A case with several incidents suggestive of tubercle, and apparently taking origin from a caseating femoral gland, the result of a blow—Great improvement under simple treatment, but evidence of omental thickening, and disease of one pulmonary apex still present after three months.*

John P., an emaciated boy, eight years of age, was admitted to the Western Infirmary on the 18th January, 1884, and dismissed on the 20th of April in the same year, so much improved in all respects that, whether as regards the general condition or the local phenomena, it might very truly have been said at the time of his leaving us, that "you would hardly have known him." Like the other cases detailed, he had for his chief or most obtrusive symptom on admission a swollen abdomen, which measured  $29\frac{1}{2}$  inches in its extreme circumference on the 20th of January (two days after admission), and afterwards declined so rapidly, that exactly three weeks afterwards (9th February) it was 4 inches less; the other changes noted in great detail in the journal (but of which I will spare you the particulars) showing that in the interval a considerable quantity of fluid, shown to be present on admission both by the percussion signs and by a very manifest fluctuation, had been nearly or completely absorbed, exactly as in the case of the little girl of like age, Jane M. Moreover, there were some further points of resemblance between these two cases, the statement of which here will save me, to a great extent, from the necessity of going over the same ground again in this brief narrative. As long as fluid was present in appreciable quantity, it was impossible to be quite sure of the changes in the wall which became apparent afterwards; only it could be remarked very easily and certainly that the physical changes were not those of simple ascites to a like amount, the dulness on percussion over the whole right side of the abdomen, and even on the left side in the

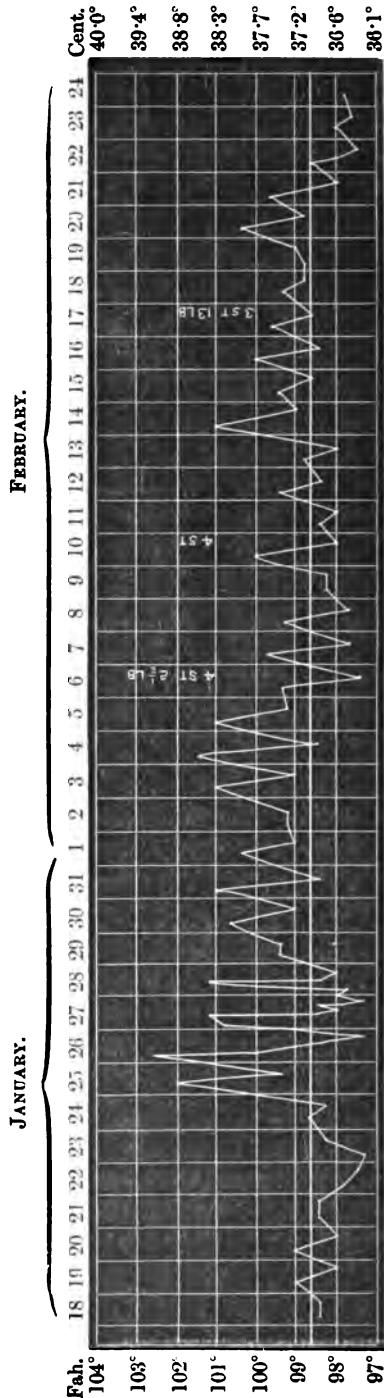
umbilical region, being inconsistent with the perfectly free movement or floating up of the intestines, as we have already said. Only when the fluid effusion was nearly or altogether gone did it become evident that there was an area of relatively dull superficial percussion extending across the abdomen in the umbilical region, and with this (at date 9th February, twenty-three days after admission) "a feeling of decidedly increased resistance on the right side of the abdomen especially, and even a certain amount of irregularity as of a transverse induration just about the level of the umbilicus and for an inch and a half above it, corresponding exactly with the type of a thickened and retracted omentum; and it is noteworthy (the report proceeds) that the dull percussion, though perhaps everywhere more apparent on the right side (it had been according to a previous report dull all over on the right side) is at this level decidedly greater—*i. e.*, less easily penetrated." (You will remember the significance that I attached in the other cases to this fact.) "The left hypochondrium and epigastrium are now more approximately normal than formerly (*i. e.*, than eight days before, when it was doubtful if the percussion even over the stomach was quite normal), and it is only from about  $1\frac{1}{2}$  inches below the hypochondrium downwards, that delicate manipulation makes out abnormal percussion superficially, which continues from this point to the lower abdomen, being, however, very easily penetrated a little to the left of, and below, the umbilicus. On this side also, there is a feeling of undue and unequal resistance, which may very well be conceived to correspond with a thickened great omentum, but by no means so definite or so limited in area as on the other side." What gave these facts a quite peculiar interest for the clinical class at the time was that if they were not declared in advance, they were at least sought for as the probable and (as it were) normal results of the antecedent conditions, and accordingly every care was taken, by repeated comparison with healthy subjects and with the previous reports, to guard against every possible fallacy in observation.

Thus far, then, the case of John P. corresponds so much in its details with that of Jane M., that we may consider the local details of the physical facts in the abdomen as sufficiently clearly indicated. There were two other points of resemblance between these cases. This little boy, like Jane M., suffered, as we were told, no pain in his abdomen throughout; indeed, he suffered less pain, if possible, than

# TUBERCULAR PERITONITIS.

## CHART OF TEMPERATURE.

JOHN P., ET. 8.



Jane M., though he was much more of an invalid, and, as we shall see, much more severely *ill*, altogether. Then, in the family history there was one other fact common to the two cases; this boy had, among six brothers and sisters, one who was said to have been ill of, and in fact to have died of, "a wasting of the bowels." I give the popular expression for what it is worth, in both cases: we know, and are likely to know, nothing more of the facts, or of the family history, in either case.

But with these facts resembling, or common to the two cases, there were also very considerable differences. One was as regards the temperatures, which in the former case were scarcely ever more than just appreciably abnormal, and this only for a very brief period, while in John P.'s case they once reached  $102.6^{\circ}$  F., and were on repeated occasions at upwards of  $101^{\circ}$  within the first three weeks after admission, the temperatures so elevated being almost always the evening temperatures, while the morning temperatures were approximately normal, and the daily oscillation therefore amounted to two or three degrees of Fahrenheit's scale (one and a half to two or more C.), there being in one instance a fall of  $5.4^{\circ}$  F. during a single afternoon and night—*i. e.*, from  $102.6^{\circ}$  to  $97.2^{\circ}$  F., or from a very decidedly feverish to a very decidedly subnormal temperature. These are typical facts for a large proportion of cases of this kind, and accordingly you might almost take the temperatures of five or six weeks in this case, here recorded on a chart or diagram, as being a clinical working model for what is to be expected in a well defined case of chronic or sub-acute peritonitis in a child, when it is only moderately severe—so severe as to justify apprehension, but not so severe as to exclude hope of great and marked benefit through care and treatment. In this case, the abnormal temperatures gave way after six weeks' residence, and all the other symptoms underwent a corresponding improvement; so much so that, but for the very decisive local lesions, you would almost have been justified in doubting the serious or dangerous character of the case. But although, as we have said, a typical case so far, it is also true that in many other diseases of the infantile period, you may have temperatures not essentially differing from those of chronic peritonitis; and it is also probable that you may have (though rarely, I believe) a chronic peritonitis in which the temperatures are never appreciably disturbed at all.



There were, however, other circumstances in the case now reported to you, which gave to it a much graver aspect in my eyes at the time than the case of Jane M., and thus enabled me to feel a corresponding sense of relief, as from something happily occurring and yet unexpected, when the course of the symptoms showed so decidedly, after six weeks, that we were on the line of a proximate, if not an absolute *cure* (popularly so-called) of this disease. One ominous-looking fact was that the serous or sero-fibrinous effusion was not limited to the peritoneal cavity, a very considerable amount of dropsical swelling of the penis and scrotum having taken place about a week after admission; this being, too, altogether without swelling of the feet, or any of the other concomitants of general dropsy, but still, on this very account, an exceptional fact, and one suggestive of lesions in the venous and lymphatic systems of the pelvis. Another fact was the presence of diarrhoea (as in the case of Mary Jane S., but not so severe) during the second week of the abdominal swelling. A third fact was the presence of a lesion, very distinct in its physical signs, and not without significantly ominous symptoms—"cough, unattended by expectoration, and sweating about the head, especially in the night-time, which had accompanied the progress of the more obvious abdominal signs from the first—*i. e.*, for about three weeks before admission." The physical signs in question were:—"slight dulness on percussion at the left apex, over the two uppermost ribs in front;" respiratory murmur in this situation "loud, harsh, and with a slightly tubular quality" (as compared even with the normally puerile R. M. all over); this abnormal quality extending slightly to the apex behind, but without dull percussion, or any *râle* either behind or in front. All the other organs, so far as they could be observed accurately, were normal to physical diagnosis; but a difference like this in the respiratory murmur of the two apices is so significant of constitutional infirmity, that even if we had no other sign of disease, and no appreciable symptoms at all, we should be justified in looking upon it as a ground for suspicion of more or less chronically impaired health, especially in a child or young person.

But perhaps the most exceptional of all the points of difference between the case of John P. and that of the girl of the same age, Jane M., who made such a good recovery, was one fact in the previous history which I have reserved

to the last, because it involves a doubtful question of ætiology, and also one of pathology. In Jane M.'s case, beyond a vague statement that she had always had rather a large abdomen, there was nothing at all to indicate any local lesion, or any well-established cause of possible injury to the constitutional health, up to the invasion of the disease for which she was admitted. In the case of John P., on the other hand, his mother distinctly attested (as she believed) the origin of the child's ill-health from a local injury quite outside the abdomen, and only connected with it indirectly through the vascular and lymphatic systems. He was a very healthy boy, she declared at our first interview with her, till four months ago, and had no illnesses beyond what are common to most children. "At this time he received a blow on the upper part of his right thigh, which was very soon followed by a round and painful swelling, the size of a walnut, among the soft parts in the femoral region; but after a few days the pain became less, and the swelling also diminished. It has since remained quite stationary and free from pain. After this injury, the boy never seemed to recover perfect health. He lost appetite, became paler, weak, and emaciated. These symptoms went on till about three weeks before admission, when a general swelling of the abdomen was observed, which has gone on increasing until the present date (20th January, two days after admission). This swelling was not accompanied by any abdominal pain, but there has since been increasing weakness and emaciation, &c., the cough and hectic fever being presumably of the same date, and probably of identical origin with the abdominal disease." Taking these facts in connection with the rather unusual fact above referred to, of dropsy of the scrotum and penis, suggestive of local venous obstruction in the pelvis or lower abdomen, it is difficult to avoid the conclusion that an inflamed and caseating gland at the seat of the blow had been in some way or other the starting-point of all the subsequent lesions; a theory, as you know, which has been in great favour in Germany for many years past as bearing on the causation of tuberculosis; and which, although I do not adopt it in the extreme sense of Niemeyer, for example, as regards phthisis pulmonalis, is certainly well fitted to explain, so far, the order of events in this case. Here is the actual description of the facts, as they were observed, in connection with the preceding history, a few days after admission.

January 20th.—“The swelling described as the first incident of his illness is rather less than a walnut in size, situate in Scarpa's space on the right side, about two inches below Poupart's ligament. On handling, it gives at first the impression of being soft and elastic, but deeper palpation leads rather to the belief that it consists of a solid base, the more superficial part being so highly elastic as almost to suggest fluctuation. The skin over it does not differ in appearance from the neighbouring skin. In the right inguinal region several of the glands, chiefly belonging to the chain running in the direction of Poupart's ligament, are distinctly enlarged.” And, again, on February 1st, “An examination of the usual sites of glandular enlargements shows an entire absence of these in the neck, axillæ, and left groin. In the right upper femoral region there is a soft, elastic, not decidedly fluctuant swelling, between the size of a hazel-nut and that of a walnut, fairly mobile as regards the superficial textures, and presenting no pointing or other sign of an abscess, but seeming to be attached to the deep fascia with considerable firmness. It is painless on handling, but the history quite definitely leads to the belief that in the first instance it was caused by a blow, was painful, and considerably larger than it is at present. Dr. Gairdner finds a difficulty in determining how far this swelling is glandular, but several of the glands over Poupart's ligament are slightly enlarged, without any inflammatory thickening of the surrounding textures or pain on pressure.” As this swelling, whatever it was originally, did not frankly suppurate during the whole time this patient was under observation (three months), it is certainly most probable that a caseating process had taken the place of active suppuration, if indeed the latter at any time existed.

The latest information I have to give you as to the progress of this case under observation, and, I may say, under a very simple treatment, much resembling that of Jane M., is contained in the following report of 8th April, which I think it worth while to present to you exactly as it appears in the journal, and which I think you will agree with me in considering, under the circumstances stated, very satisfactory.

“Since the 1st February, at which date a general survey of the temperatures was recorded, there have been on eight evenings temperatures noted extending up to, or over, 100°, the maxima being on February 4th, 101·5°; on

14th, 101°; and since this barely exceeding 100° in any instance; while the great majority of the temperatures, especially since the first week of February, have been subnormal. A very distinct, though gradual, gain in weight, and a very marked filling up of the features as compared with his state on admission, has been observed throughout this period. The urine, moreover, has undergone very decided and apparently permanent increase as compared with the state on admission and during the period of febrile temperatures. The last ten daily collections recorded from 21st to 31st March give 28 oz. as a mean. This mean quantity was even exceeded about the middle of February for some days together, 31 oz. mean being recorded in six days from 21st to 26th February, but on the whole the above may be considered to present a fair average for the last six weeks. The last weight recorded (April 7th) was 4 st. 4 lb. The boy seems to suffer no discomfort at all either from the abdominal symptoms or from the swollen gland in the right groin, which seems to be slowly maturing towards abscess, and presents a very distinct red blush at the most elevated point of the skin.

"The conditions in the abdomen as last described are not very essentially altered, except that the distinct feeling of transverse, almost solid impaction to the right of the umbilicus, has given way to a more diffused and less definite sense of increased resistance, with superficial dull percussion, however, almost equally characteristic of a thickening extending over a transverse zone about three inches in vertical diameter, half of this being above and half below the umbilicus. A nearly corresponding dulness on the left side is also much less in degree and in limits. Tongue perfectly clean. There has been no diarrhoea at any time. There is still distinctly impaired percussion in the left pulmonary apex, and the R. M. is relatively less full than in the opposite; but the alteration in quality is so slight that but for the other facts it would certainly be disregarded."

On the 20th of April, 1884, as above stated, John P. was dismissed from the Western Infirmary very much improved in health and in general condition.

When Cases II and III are carefully considered in connection with Case I, it will be found that, although differing

in their prognosis and probable issue,\* they are cases, in many respects, of the same order. In all of them there was observed the presence of a very notable quantity of fluid effusion in the abdominal cavity, with physical conditions differentiating the case from simple ascites, and leading to the inference of lymph effused so as to restrain the free movements of the intestines; while in two of these cases at least the physical signs after the removal of the fluid were exactly those of thickening of the great omentum. Each case, in fact, is a typical illustration of one or other feature in the above diagnosis; and while in Case I, taken by itself, the complete and satisfactory curative result might induce a doubt as to the pathology, and lead to much hesitation in admitting the case into the category of a *tubes*, the other two cases, in various degrees, may be said to fully deserve this designation. The case now to be submitted (Case IV) is a much more remarkable one than any of the preceding; and how far it can be said to fall within the description of a *tubes* may be held over for further discussion.

(To be continued.)

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## CURRENT TOPICS.

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**BRITISH MEDICAL ASSOCIATION.**—A meeting of the General Committee, to make arrangements for the annual meeting of the British Medical Association in Glasgow next year, was held in the Faculty Hall on the afternoon of Wednesday, the 12th ult.—Professor W. T. Gairdner, President-Elect, in the chair. After the minutes of the previous meeting had been read, the Chairman explained what had been done by the Provisional Executive Committee since the meeting of the General Committee in the month of May last. Drs. Yellowlees and Coats, Hon. Treasurers, had instituted a subscription list for the Guarantee Fund; and although something over £1,000 had already been promised, this was considerably below what these gentlemen had expected and what was necessary to complete a guarantee fund of £1,500. At the Dublin meeting, in August last, a formal invitation to the Association to hold

\* I have not been able to trace the boy John P., although inquiries have been made, by post and otherwise, at his former residence. The facts recorded must be taken, therefore, for what they are worth as indicating a possible recovery. It is to be feared that the improvement may have been only temporary.

the 1888 meeting in Glasgow had been given, and had been very cordially accepted. At that time the Council of the Association had wished the Glasgow members, who tendered the invitation, to nominate nine gentlemen to form part of the General Arrangements Committee to meet in London; but Dr. Gairdner and those acting with him felt that they were not in a position to do this at the time, and so it would be one of the duties of the present meeting to elect these gentlemen. Dr. Glaister next explained the duties of the various Sub-Committees which fell to be appointed at this meeting; and on the motion of the Chairman, the following were unanimously appointed:—

#### EXECUTIVE COMMITTEE.

*Chairman*—Dr. Yellowlees. *Hon. Local Secretaries*—Drs. Mackendrick, Christie, and Glaister. *Hon. Treasurers*—Drs. Coats and J. B. Russell. Messrs. Henry E. Clark and A. E. Maylard; Drs. R. S. Thomson, C. F. Pollock, Erskine, Eben. Duncan, W. G. Dun, J. W. Anderson, James Hamilton, Leishman, James A. Adams, Thomas F. Gilmour, John Barlow, Thomas, Wm. Muir, Sir George H. B. Macleod, Dr. Middleton, and Dr. Beatson.

#### SUB-COMMITTEES.

I. MUSEUM.—*Chairman*—Mr. Henry E. Clark. *Hon. Secretaries*—Mr. A. E. Maylard and Dr. R. S. Thomson. Drs. A. M. Buchanan, Finlayson, Macfie, Macewen, Macintyre, Newman, Robert Pollok, M'Gregor-Robertson, and Thomas Reid.

II. PRINTING AND PUBLISHING.—*Chairman*—Dr. Christie. *Hon. Secs.*—Drs. C. Fred. Pollock and Erskine. Drs. A. L. Kelly, Knox, Napier, Pirie, Russell (Western Infirmary), Lindsay Steven, and one secretary from each of the other sub-committees.

III. LOCAL ENTERTAINMENTS.—*Chairman*—Dr. Eben. Duncan. *Hon. Secretaries*—Drs. W. G. Dun and J. Wallace Anderson. Drs. Allan (Belvidere), Barr, Barrae, John Burns, Fraser (Paisley), M'Phail (Whifflet), Mather, Morton, Alex. Patterson, Wallace (Greenock).

IV. EXCURSIONS.—*Chairman*—Dr. John Glaister. *Hon. Secretary*—Dr. James Hamilton. Drs. Barlow, Charteris, Neil Carmichael, John Dougall, Yule Mackay, M'Vail, Stirton, H. B. Wilson, and Robertson (Dumbarton).

V. DINNER AND LUNCHEON.—*Chairman*—Dr. Leishman. *Hon. Secretaries*—Drs. James A. Adams and Thomas F. Gilmour. Drs. George Buchanan, James Dunlop, Wm. J. Fleming, W. L. Reid.

VI. SOIREE.—*Chairman*—Dr. Coats. *Hon. Secretary*—Dr. John Barlow. Drs. John Alexander, J. W. Anderson, Campbell Black, Freeland Fergus, Gairdner, Gemmell, Bruce Goff, Lapraik, Wood Smith, Whitson, Bruce Young.

VII. GENERAL PURPOSES.—*Chairman*—Dr. Thomas, Royal Infirmary. *Hon. Secretary*—Dr. William Muir. Drs. T. Beath Henderson, Maclaren, Robert Park, and Perry.

VIII. RECEPTION.—*Chairman*—Sir George H. B. Macleod. *Hon. Secretaries*—Drs. Middleton and Beatson. Drs. M'Call Anderson, Hector C. Cameron, Samuel J. Moore, Morton, Ed. M'Millan, Joshua Paterson, Renton, Alex. Robertson, P. A. Simpson, Samuel Sloan, George Turner, Wolfe, A. Wallace.

On the motion of Professor M'Kendrick, seconded by Dr. J. Lindsay Steven, the following nine gentlemen were elected members of the General Arrangements Committee to meet in London—viz., Drs. Gairdner, Coats, Duncan, Finlayson, Glaister, M'Kendrick, M'Vail, Morton, Yellowlees. Dr. Yellowlees was appointed Chairman of the Executive Committee, and Dr. J. B. Russell was elected an Hon. Treasurer in his stead. Dr. Christie moved the appointment of the Executive Committee, which consists of the Chairman, Hon. Secretaries, and Treasurers, and the Chairman and Secretaries of the various sub-committees. Dr. Joseph Coats then made a statement on the subject of finance. Up to the 10th October, 1887, £1,064, 11s. had been subscribed, and of this the country subscriptions amounted to £63, 4s. At least about £500 more would be required, and, as the present statement showed, it would be necessary, with this object in view, to put forth renewed and vigorous efforts, especially in our country districts. We have no doubt that the very laudable desires of the Honorary Treasurers will be amply satisfied, and that during the winter months subscriptions will come flowing in from the West of Scotland generally, for we are confident that the urgency of the matter requires only to be properly represented to our country colleagues in order to ensure a most generous response.

THE OCTOBER MEDICAL EXAMINATIONS AT THE UNIVERSITY.  
—The preliminary examinations for medical students, held early last month, were attended by an exceptionally large number of students, only a few short of 400. The total number of rejections was, however, very great, considerably over 50 per cent. The number of men desirous of entering the medical profession seems always to be increased in times of commercial depression, and it is natural, under such circumstances, that many should present themselves for admission as students, who from their previous training and habits are totally unfitted for such a career. It is only right and merciful that such men, by a stringent preliminary, should be turned back at the outset, and be prevented from an expenditure of time, labour, and money, which can only end in failure. The number of candidates for the various professional examinations has also been considerable—viz., for the first 131, for the second 80, for the third 82. We subjoin a list of the gentlemen who have been successful in passing these examinations.

The following gentlemen have passed the first profes-

sional examination for the degrees of M.B. and C.M.:—Kilgour Adamson, Alexander Percy Agnew, Charles Angus Alexander, James Alexander, James Gordon Bain, William Barbour, James Barr, John Fulton Barr, Robert Burns, Matthew Cameron, James Hamilton Campbell, John Munro Campbell, William Cassels, James Chalmers, James Easton Chalmers, Andrew Cluckie, John Cochrane, Samuel Cockburn, David Coutts, Peter Drummond, M.A.; George Henry Edington, David H. Fotheringham, M.A.; James Fisher Frew, John Gilmour, James Jarvie Hood, Anthony Inglis, Gilbert P. Johnson, James Kirkwood, Matthew Lochhead, James Rowatt Logan, George Agnew Main, William Mason, James Nelson Matthews, Alexander Goodsir Mowat, James Muir, Donald Murray, Colin M. Macaulay, David M'Donald, William Dove Macfarlane, Duncan Otto M'Gregor, Hugh Macintyre, Daniel M'Kenzie, James Paterson MacLaren, John Norman Macleod, M.A.; James Gibson M'Naught, M.A.; Peter M'Ritchie, Robert James Nevin, Robert Nichol, William Park (Beith), William Park (Kilmarnock), Edward Ley Paton, M.A.; John Purdie, Brownlow Riddell, William Robertson, Arthur T. Scott, Charles Francis Scott, William Shearer, Robert Sharp, David Smith, William M'Gill Smith, William Caldwell Steele, Robert Stevenson, James Stirling, Roderick Tate Sutherland, Lewis Dunbar Temple, Sholto Douglas Thomson, David Watson, John Watt, James Alexander Wilson, James Allan Wilson, Robert Wilson, Andrew Wylie.

The following have passed the Second Professional Examination:—Thomas D. Bertram, Henry M'D. Borland, John C. Bowie, Robert Broom, David Brown, Mungo Bryson, William Butler, John Angus Cameron, John Crawford, John Cunningham, Archibald Donald, Neil Downie, Alexander C. Farquharson, James Fisher, Isaac Fletcher, Thomas Forrest, Laurence Fraser, William Gemmel, James Gilchrist, Malcolm Gillies, Walter Groome, Thomas Arthur Haig, John Hardie, Frederick Hare, Andrew Biggam Houston, Walter King Hunter, Robert Kennedy, Robert Robertson Kilpatrick, Thomas Brough Law, James Lloyd, Arthur H. Lucas, Robert Miller, Andrew Ronald Mitchell, Thomas D. Moffat, William Murray, Robert F. Macarthur, M.A.; Anthony M'Call, Robert C. Macdiarmid, John Macdonald, Patrick Fraser Macgregor, Donald MacKenzie, John M'Kie, Robert M'Lay, Robert Alexander M'Lay, James Andrews Macpherson, Robert Alexander Paton, Arthur Alfred Pratt, Alexander Prentice, Ferdinand Rees, Thomas Boyd Riddall, Andrew Robertson, John Robertson, Douglas Wills Russell, William J. M. Slowan, John Cockburn Syson, George Marius



Edwin Thorp, William R. K. Watson, M.A.; Adam C. White, John Wotherspoon.

The following have passed the Third Professional Examination : \*—William Edlund Lawrence Allen, (a) William Auld, Thomas Crawford Barras, (a) James Bauchop, James Paton Boyd, Robert Charles Brodie, Alexander Buchanan, (a) Robert Calderwood, Finlay Stewart Campbell, James Cook, James Culross, M.A.; (a) Andrew Davidson, M.A.; (a) John Donald, M.A.; Donald Ferguson, M.A.; (a) Charles James Fyfe, (a) Edward Henry Fyffe, Peter Gardiner, David C. Gray, Andrew Halliday, Robert Taylor Halliday, (a) Yousuf Hamis, (a) John C. Howie, M.A.; Alexander Marmaduke Hutton, Robert G. Inglis, (a) Thomas Houston Jackson, Robert Jamieson, John M. Johnston, (a) Adam Brown Kelly, John Gilmour Kerr, (a) Robert Bain Lothian, (a) Joseph Bateman Morton, James M. M. Muir, Daniel M'Dougall, (a) John T. M'Lachlan, Thomas M'Murray, (a) Daniel M'Nicol, (a) John Wyllie Nicol, (a) Landel Rose Oswald, (a) William A. Parker, (a) William Primrose, David Ramsay, Charles E. Robertson, William Roxburgh, James Allan Stewart, William Stewart, James Strang, (a) Lewis Robertson Sutherland, John Porter Tannock, (a) John Taylor, (a) Robert Weir, William M. Wilson, John Wright.

**MEDICO-CHIRURGICAL SOCIETY OF GLASGOW.**—At the first meeting for the session, held on the evening of the 7th ult., the following gentlemen were elected office-bearers for session 1887-88 :—*President*, Professor M'Call Anderson, M.D.

*Section of Medicine.*—*Vice-President*, Dr. D. C. M'Vail; *Council*, Dr. S. Gemmell and Dr. J. W. Anderson; *Secretary*, Dr. George S. Middleton.

*Section of Surgery.*—*Vice-President*, Dr. A. Patterson; *Council*, Mr. A. E. Maylard and Dr. David Knox; *Secretary*, Dr. John Barlow.

*Section of Pathology.*—*Vice-President*, Dr. Joseph Coats; *Council*, Dr. Alex. Robertson and Dr. Wm. Macewen; *Secretary*, Dr. T. K. Dalziel.

*Section of Obstetrics.*—*Vice-President*, Dr. James Stirton; *Council*, Dr. E. Macmillan and Dr. A. Wallace; *Secretary*, Dr. Thomas F. Gilmour.

*Treasurer*, Dr. Hugh Thomson; *General Secretary*, Dr. Wm. G. Dun.

After the election, a paper on "Substitution in Disease, &c.," was read by Dr. Alexander Robertson, the discussion of which,

\* Those whose names are marked (a) have not been examined in pathology.

owing to the lateness of the hour, was deferred till the next meeting of the medical section.

On the evening of Friday, the 14th October, a *conversazione* of the Society, to which all members of the profession in Glasgow and neighbourhood were invited, was held in the Faculty Hall. Professor McCall Anderson, the lately elected President, presided, and a very large number of medical practitioners accepted the invitation of the Society, and we were specially pleased to see so many of our country brethren. All the rooms of the Faculty, in many of which objects of artistic and antiquarian interest were shown by different members, were thrown open to the Society's guests, who spent much time in examining the curiosities exhibited. Music was provided for the entertainment of the assembly by a small but efficient orchestra in the large hall, where at intervals during the evening recitations were given by Mr. Baynham, whose rendering of "Bob Sawyer's Supper Party" the audience seemed very specially to enjoy. The gathering was a real *conversazione*, the flow of talk being interrupted only by the music and the readings. From the very manifest interest with which the objects provided for inspection were examined, and from the hearty greetings which, on all sides, were heard passing between old friends who had perhaps few opportunities of meeting, one could not help concluding that the meeting was a decided success, and wishing that before very long a similar gathering would be held.

GLASGOW PATHOLOGICAL AND CLINICAL SOCIETY.—The first meeting for the present session was held on the evening of Monday, the 10th October last. The President, Dr. James Finlayson, delivered an address on the use of medical societies at the present day and the abiding importance of pathology to clinical medicine. Demonstrations on bacteriology, laryngoscopy, and photomicrography were also given by Mr. A. E. Maylard, and Drs. Newman and Thomas Reid.

GLASGOW SOUTHERN MEDICAL SOCIETY. — The annual business meeting of the members of this Society was held on Thursday evening, 13th ult., in their rooms, 11 Bridge Street, when the following office-bearers were appointed for the ensuing session, 1887-88:—*President*, Edward McMillan, L.R.C.S.; *Vice-President*, John Glaister, M.D.; *Treasurer*, T. F. Gilmour, L.R.C.P.; *Secretary*, James Erskine, M.A., M.B.; *Editorial Secretary*, R. A. D. Robb, M.B.; *Seal-Keeper*, James Hamilton, M.B.; *Court Medical*, James Morton, M.D. (*Con-*

vener); James Dunlop, M.D.; J. Stuart Nairne, F.F.P.S.; David Tindal, M.D.; David N. Knox, M.A., M.B.; *Extra Councillors*, Drs. John Brown, David Couper, and James Johnstone.

THE SCOTTISH MIDLAND AND WESTERN MEDICAL ASSOCIATION.—The Sixteenth Annual Meeting of the members of the above association was held in the Religious Institution Rooms, Glasgow, on Friday, 14th October, 1887—Dr. Caldwell, Shotts, President of the Association, in the chair. After the election of office-bearers for the year 1887-88, the President gave his inaugural address on "Reminiscences and Reflections after Thirty Years' Practice." After thanking the members for the honour conferred he said that, as one who had borne a little of the heat and burden of the day, he hoped that the selection of a subject would not be considered out of place on the present occasion. In his notes made while acting as a dresser in the Royal Infirmary during 1851-52, he found that hospital gangrene, phlebitis, and erysipelas were then prevalent, and that while sepsis, asepsis, antiseptics, and drainage might perhaps have been thought of, they had not at that time been reduced to practice in any form. Reference was also made to the fact that the late Professor Lawrie was in the habit of using pounded ice as a local anæsthetic, and that, at the same period, the late Dr. Perry, the late Professor Easton, and others, took much pains in pointing out the differential diagnosis between typhus and typhoid fever, and that Sir William Aitken, then pathologist to the Infirmary, demonstrated the lesions (*post-mortem*) that took place in the fatal cases of typhoid fever. The next point taken up was some noted cases in the village of Dreghorn. During 1857, two cases in particular call for special notice, as they are interesting cases of insanity. The first is the case of J. R., æt 31, height about 5 feet, 10 inches; a strong, muscular young man, generally inoffensive and quiet. J. R. was in the habit of collecting a bagful of small stones; he would take them to the centre of the road, put one down carefully, and then with a powerful kick from his strong boot would send the stone rattling down the road. The harder the stone rolled, the louder and longer John would laugh. After the stones were all disposed of, John would quietly gather them all up again, put them in a snug place, and go home well pleased with his day's performance. The second case is that of Old Tom, a stout little man of about 5 feet, 7 inches in height, and supposed to be about 50 years of age, with a well

marked English accent. He always wore a blue ribbon in his coat, and was never seen without a carter's long whip in his hand. When Tom could procure a cart wheel he would race along the public road with his wheel, cracking his whip in great style. In 1863 Tom died of acute pleuro-pneumonia of three weeks' duration. The villagers say that Tom was the son of an English farmer; that while he was driving a horse and cart with a sister in it, the horse ran away, and his sister was killed, and that Tom was never seen in England after the sad event. However that may be, Old Tom was a harmless lunatic; he wore his ribbon, cracked his whip, and ran his wheel about Dregghorn and Irvine for over thirty years. Leishman's paper in the *Glasgow Medical Journal*, in 1860, on supporting the perineum, and then, in 1864, his essay on the mechanism of parturition, called for more than a passing notice, and reflect much honour on the Glasgow medical school. New medicines, new operations, and new appliances were next noticed. The medical student of to-day is quite a different person to what he was 30 years ago. No need now for the characters of Dickens and of Albert Smith, as Bob Sawyer and Ben Allen have made their "*exceunt*" long ago. It is a pity students are not taught phlebotomy and cupping, as they used to be. After a somewhat lengthy paper, the President concluded with the following words:—"Let us cultivate a high ideal of our profession. Let us see in each of our patients, young or old, a reflection of self, a conscious being, who shrinks from pain, who enjoys living, and who has longing for a better, higher, and nobler existence than the present. 'We live in deeds, not years; in thoughts, not in breaths; in feelings, not in figures on a dial. We should count time by heart beats. He most lives who thinks most, feels the noblest, acts the best.'

'This above all—to thine own self be true,  
And it must follow as the night the day  
Thou canst not then be false to any man.'

A most interesting and lively discussion followed the reading of the paper, in which Dr. Messer, Helensburgh; Dr. Boyd, Slamannan; Dr. Loudon, Hamilton; Dr. Lindsay, Lesmahagow; Dr. Willis, Baillieston; and Dr. McGown, Bellshill, took part. The members afterwards dined in Forrester's Restaurant, and spent a very happy evening.

KILMARNOCK.—FAREWELL DINNER AND PRESENTATION TO DR. MACFARLANE.—On the evening of 20th September, Dr.

A. W. Macfarlane, who is about to begin practice in London, was entertained to dinner by a number of his patients and medical brethren. Sir William H. Houldsworth, M.P., occupied the chair, and G. A. Baird, Esq., M.P., acted as croupier. Among the toasts that of "The Scottish Universities," proposed by the Croupier, was responded to by Professor Grainger Stewart; and that of "The Medical Profession," by Professor Gairdner. The testimonial consisted of (1) a handsome silver gilt dessert service, comprising 13 pieces, and weighing over 260 ounces; (2) an antique buhl chiming clock and a diamond pendant (for Mrs. Macfarlane) containing 30 brilliants. In making the presentation, the Chairman referred in complimentary terms to the high esteem in which Dr. Macfarlane had been held by his many patients in this locality. The Doctor replied in a graceful speech, and, after the various toasts had been proposed and responded to, a very successful meeting was brought to a close.

ON STROPHANTHUS AND ITS PREPARATIONS.—We have received the following interesting note from Messrs. Burroughs, Wellcome & Co.:—The introduction into commerce of this remarkable drug affords a striking illustration of the facilities which commerce renders at the present time in obtaining from the most distant parts of the world such rare drugs as medical science may require. Dr. Livingstone was about the first to describe the plant and its uses, but it was many years before Professor Fraser was able to obtain enough of the drug upon which to complete his labours and exhaustive experiments. When his reports were published there was none of the drug in the market, but realising the urgent demand that must arise for it, we sent to various parts of Africa at very great expense, and bought all that we could obtain at most exorbitant prices, as the pods were what the natives had collected for their own use to poison their arrows with. The quantity, however, was so small that, in order to enable tests to be made, by as many medical men as possible, with tincture prepared from it, we supplied only one half ounce bottle on any one order. Some physicians, who had remarkable results from its use and were determined to continue prescribing it, obtained supplies in a very ingenious manner by having several medical friends each order a bottle. Even at the high price of 2s. per half ounce bottle, our expenses on this initial lot nearly doubled our returns. Upon the arrival of our next importation of strophanthus seed, in prime condition, the price was very considerably reduced.

Our agents spared no trouble to obtain the full quantity of seed required, and we were then enabled to prepare a tincture superior in quality to former lots. Now tincture of *strophanthus*, which had previously been so costly, is about as inexpensive to administer as any tincture of the pharmacopœia, showing how rapidly the resources of commerce in modern times are able to supply anything that may be required from the most remote parts of the world. The tincture which was sold at the rate of 64s. per lb., twelve months since, we now supply at 9s. per lb.—15s. per dozen one ounce bottles and 9s. per dozen half ounce bottles. For convenience of administration, and to insure accuracy of dose, we prepare tabloids containing two minims each of the tincture, which are now dispensed at the ordinary price of pills or powders, and are supplied by us at 1s. 6d. per bottle of 100 to the profession. It will therefore be of interest to the medical profession to know that the cost of *Strophanthus* is no longer a hindrance to its employment in cases where it is indicated.

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## Obituary.

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### JAMES THOMAS CARTER.

A SHOCK seemed to run through the profession in Glasgow when, a few weeks ago, a rumour spread that Carter of the Western was dead. It was touching to find in what respect he was held throughout all ranks. The men who did not know him personally, at least knew of him, and had some good word for his memory.

Mr. Carter was born and bred in Manchester. At school he carried off Queen's prizes in almost all the physical and natural sciences. The first of these was won when he was only eleven; and, significantly enough, it was for physiology. His wish at this time was to enter at Oxford. But it could not be, and he had to go into an office. Owen's College, however, had its evening classes; and there he continued his studies, devoting most attention to chemistry. But he found time to master French and Spanish, and these languages were at his command to the end—though he seldom dropped a hint of the breadth and depth of his knowledge. Of course he got on. And when his resignation was reluctantly accepted, he gave up a place of trust and the duties of foreign correspondent.

Mr. Carter's second nature had broken through. He bought land in New Brunswick, and in his pine clearings by the Atlantic, lived, as he used to say, "a free life." With rod, rifle, and books, Carter led a life that he never spoke of without a kindling eye. But in time he thought of his children, and made up his mind. He went to Halifax, N.S., and put in his first year in medicine. Then he crossed to Edinburgh, distinguished himself at once in the extra-mural school, and rose to the head of the staff at the Minto House School of Anatomy. After one session of work as a qualified man, he returned to New Brunswick—this time for the sake of his family—and very soon got into good practice in Dalhousie. But Carter was not a Puritan. A man of purer, sweeter, or more upright life could not easily be found. But his was not the Puritanism of a village in New Brunswick. Though possessed of only a modest competence, he accepted the proffered lectureship on anatomy in the Western Medical School, and began work there four years ago—with nothing in his favour, and a good deal against him. Every one knows how he succeeded as a teacher, only one or two know what he did as a student. Because he could not draw very well on the blackboard, he went to the School of Art and began at the beginning. He would leave the hard work of his own rooms, and cross the road to hard work in the University laboratories. Because his quick Lancashire tongue contrasted oddly with local Scotch, he was going to join an elocution class. In order to take his degree in science, Mr. Carter had been working very hard—harder by far than he allowed his friends to know. And there is evidence that he had worked up to far above an honours level. Unfortunately, he overtasked himself. He had a delicate stomach, and, reading hard, was apt to avoid the nuisance of dyspepsia by living on little more than a biscuit. Last summer he went too far with this hard thinking and meagre living, and in the autumn Nemesis overtook him at a lonely and remote clachan in the West. Every summer the old half Indian longing for the open came upon him; and this year he unfortunately went to a place where even common comforts are hardly to be had. He took ill, and came back in a very weak state. In a short time scurvy and pernicious anæmia developed themselves. Under treatment he did well. But on the very day that he felt himself turning the corner, collapse set in. He was dead by noon of the next day. It is not easy to write of him now, for some of us loved him. Full of native Lancashire fight, he never had an unkind

word for friend or foe. A braver, truer, gentler Englishman never stepped. He had just begun to make his mark in research, when his life withered and dropped at the very unfolding of its blossom. He has left desolate as happy a home as there was in this great city. But he has left behind him a good name, a few friends, and a wife proud of him in her grief. *Requiescat in pace.* W. L.

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## REVIEWS.

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*The Topographical Anatomy of the Child.* By JOHNSON SYMINGTON, M.D., F.R.S.E., &c. Illustrated by 14 Coloured Plates and 33 Woodcuts. Edinburgh: E. & S. Livingstone. 1887.

"WHILE the anatomy of the human subject, during the earlier months of intra-uterine life, has been investigated with great care and minuteness, and while our knowledge of the topographical anatomy of the adult has been enriched by the labours of numerous investigators, comparatively little attention has been paid to the peculiarities in the anatomy of the infant, and the changes that occur from the time of birth up to that of adult life." In these words Dr. Symington gives the reasons that have moved him to undertake and carry through the investigation of which this volume forms the very tangible result. Not only is it true that very little has been published on the subject, but it is also true that no inconsiderable part of that little was erroneous; and there is scarcely a page of this book but corrects some common error, or throws new light on anatomical relations as they occur in the child.

The mode of investigation pursued has been mainly that by sagittal, horizontal, and coronal frozen sections of the body made at different levels, but supplemented by other methods where fuller information was required. The work is divided into two sections, the first forming a running commentary on the plates, and the second a formal discussion of the points wherein differences are observable between the anatomy of the adult and that of the child.

Hitherto, anatomists have had no very clear ideas as to date of formation of the air cavities in the mastoid, frontal, and superior maxillary bones, in all of which, but more



especially the first, accurate knowledge as to their presence or absence at a given age might be of essential importance in a surgical aspect. Dr. Symington has brought much evidence to bear on the subject, and if he has not settled the question, has done much to elucidate it. He found that the *antrum mastoideum* (a cavity, as he remarks, generally overlooked by anatomists) was present at the time of birth in all the specimens examined by him, but he is of opinion that the pneumatic cavities generally are not formed in the lower part of the mastoid till about the time of puberty. This it will be seen is an important observation in connection with the question of trephining the mastoid for suppuration of the middle ear. The frontal sinuses he found in his specimens to be absent at 6 years of age and present at 9, so that we have thus pretty clearly indicated the date at which they commence to form. In regard to the antrum of Highmore, he notes that, as a rule, it opens into the infundibulum, and not, as is commonly stated, into the middle meatus, although sometimes a second opening is found leading directly into the latter.

On the much disputed point as to the position of the heart in children, he dissents from the view that in them the base of the heart is higher than in adults. He calls attention, however, to the greater relative size of the heart, and the narrowness of the front of the chest in children, and remarks that as a consequence the apex of the heart reaches to, or even beyond, the nipple line, and may be felt in the fourth intercostal space as well as the fifth.

We would note as especially worthy of careful perusal, the section on the position and growth of the larynx, that on the production of the lumbar curve of the vertebral column, and the elaborate account of Dr. Symington's researches into the development of the jaws and teeth.

The numerous references at the bottom of each page testify to the painstaking care with which the author has studied the subject, as well as displaying a praiseworthy anxiety to give full credit to all fellow-workers in the same field.

We must not omit to express our high appreciation of the clearness, accuracy, and artistic finish of the coloured plates. The numerous reference lines crossing their surface do to some extent mar their beauty, but these lines greatly simplify the understanding of the plates, and render unnecessary the usual reference tables, and to obtain so desirable a result even greater sacrifices might have been excused.

There are remarkably few errors either in the plates or letterpress. We notice that in plate vi, fig. 2, the left *pleural*

cavity is called *pericardial*, and there are a few typographical errors in the text, but none of any consequence. We congratulate Dr. Symington on the successful termination of his important investigation.

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*On the Animal Alkaloids.* By SIR WM. AITKEN, Knt., M.D., F.R.S. London: H. K. Lewis. 1887.

*A Treatise on the Animal Alkaloids, or the Ptomaines and Leucomaines.* By A. M. BROWN, M.D. With an Introduction by PROFESSOR ARMAND GAUTIER. London: Baillière, Tindall & Cox. 1887.

WE are much obliged to Sir Wm. Aitken for this brief and yet complete review of the interesting subject of the animal alkaloids, the ptomaines and leucomaines, of which we hear so much. For the sake of those of our readers who have not followed this controversy we may say that ptomaines are alkaloids evolved during the process of putrefactive decomposition from dead animal matter, while leucomaines are alkaloids evolved from the living animal tissues. These are quite analogous to the vegetable alkaloids, and indeed it appears that the latter are evolved from vegetable albumen as the animal alkaloids are produced from animal albumen.

The most important part of this work is devoted to the evolution of the alkaloids in the living body and the attempts which have been made to explain certain symptoms by the assertion that the alkaloids finding their way into the blood act as poisons. In this part of the subject, and indeed throughout, the works of Dr. Lauder Brunton are frequently referred to. The very suggestive observations by this author on the absorption of poisonous alkaloids from the intestinal canal in cases of digestive derangement are well worthy of careful consideration, and are calculated to stimulate observation and research.

We do not quite understand the objections which our author has to the "microbes" in connection with the animal alkaloids. Surely the micro-organisms of putrefaction have something to do with the evolution of the ptomaines, and it is not a violent assumption to suppose that, in the living body other micro-organisms may evolve alkaloids which will have a poisonous action on the system.

Dr. Brown's work has much more pretension than Sir. Wm. Aitken's—it is entitled a treatise. The book is divided into three parts—I. Cadaveric Alkaloids or Ptomaines; II. Vital

Alkaloids or Leucomaines; and III. The Vital or Physiological Alkaloids, the Leucomaines, in Relation to Scientific Medicine. We cannot say that the author has performed his task satisfactorily. It is true that the reader will find an account of the literature of the subject, and a statement of the chemical problems involved, but in other respects the book is too much a rather fulsome encomium on M. Gautier to give men a high opinion of the author's judgment. We find, for instance, according to our author, that "the German physician, Koch, assumes; the French physician, Gautier, demonstrates." The latter part of the book is in fact seriously blemished by such unworthy and prejudiced statements as this. We suspect that our author, although condemning freely Koch and all his works, does not know German. He speaks of M. Koch and M. Klein (on the next page Mr. Klein) as if they were Frenchmen, and the German references in the bibliography at the end have been supervised so insufficiently as to leave many errors.

We have made our remarks somewhat censorious, chiefly because we regret that a book having many merits should be so much damaged by faults which might easily be remedied. M. Gautier's introduction is written in a very temperate spirit and is well worthy of perusal.

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*A Handbook on Diseases of the Skin.* By ROBERT LIVEING, A.M., M.D. Fifth edition. London: Longmans, Green & Co. 1887.

WHEN reviewing a former edition of this work some years ago, we took occasion to speak very favourably of it. The present edition is, on the whole, worthy of the same commendation; indeed, we believe it to be the handiest and most reliable of the many smaller guides to a knowledge of skin diseases now before the profession. It is specially with regard to diagnosis, and description of symptoms and signs, that it will be found serviceable; in the matter of therapeutics it is somewhat wanting.

After the usual introduction, and a discussion of the elementary lesions of the skin, the author discourses in a refreshingly sensible fashion on one of the *opprobria* of medicine—the nomenclature and classification of skin affections. This tangled subject will be found much simplified in Dr. Liveing's pages; such hybrid names as "lupus-psoriasis" and "varicella-prurigo," so much in favour with Mr. Jonathan Hutchinson,

come in for particular condemnation, and with this view few dermatologists will disagree. Towards the end of the volume, in dealing with cutaneous ulcers, the author returns with apparent zest to this subject, and declares that "the nomenclature of ulcers is exceptionally bad;" he objects to such terms as the "healthy or simple ulcer," "the fungous," "weak," "inflammatory," "irritable," "varicose," "eczematous" ulcer, and many more. "These names are little better than jargon." He prefers to classify ulcers into two groups—ulcers arising from inflammation (caused by varicose veins, eczema, or injury) and those connected with new growths (lupus, rodent ulcer, epithelioma, lepra, syphilis).

Dr. Liveing pays the customary homage to the genius of Hebra in adopting his classification—with modifications; but even this is not perfect, as the author frankly admits, erysipelas and malignant pustule being placed among the "*non-infectious inflammations*."

Following on this we have a most interesting and instructive "arrangement of skin diseases according to their surface distribution"—an amplification of Dr. Pye-Smith's classification.

The differential diagnosis of the exanthemata meets here with unusually full consideration. Of special value are the remarks on *rötheln*, though the author has omitted to mention what is perhaps the most striking feature of a pronounced case—the absence of any proportional relation between the amount of the rash and the other symptoms. It is not uncommon to find a patient as red as a lobster from head to foot, and yet showing evidence of little or no general disturbance.

When this work was first issued it dealt solely with etiology, symptomatology, and diagnosis; in later editions the treatment of the various diseases was added, together with a special chapter on therapeutics. The author's remarks on therapeutics fall very far short of those on the other departments of his subject. He takes no notice of the recent remarkable advances in the treatment of eczema, more especially by gelatin paints, plasters, and so on. In recommending the external use of chrysophanic acid in psoriasis, he mentions only the application of ointments, which he would localise by covering with "rags and a bandage;" he says nothing of the frequently marvellous effects of salicylic acid in lupus. We are persuaded that the best local treatment of irritable and varicose ulcers (with apologies to Dr. Liveing for using these old and convenient terms) is not to keep them

constantly moist, as we are here advised; such ulcers heal much faster if kept as far as possible *dry*. Further, it has been proved that lanolin does *not* expedite the absorption of iodide of potassium.

While we thus venture to criticise somewhat freely some parts of Dr. Liveing's work, we should like to repeat our opinion that in this handbook the practitioner will find a clear, sensible, and reliable guide to the study of skin diseases. Certainly not its slightest recommendation is that it is singularly free from many of the fads of the ordinary specialist.

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*Ligaments: Their Nature and Morphology.* By JOHN BLAND SUTTON, F.R.C.S. London: H. K. Lewis. 1887.

THE evolution theory has much to answer for. It has given such prominence to the changes which have taken place, not only in species but in organisation, that biologists have come to regard the realm of nature as in a condition of constant perturbation, and to look upon every animal tissue as undergoing or having undergone metamorphosis. As a consequence, also, every morphologist seems to regard it as his bounden duty to start a new theory, no matter how wild, as to the origin of some tissue or organ. Mr. Sutton, as a representative of the most advanced school of British anatomists, cannot be left behind in this race for distinction, and hence the production of this book. The work has already been published in substance, though not in form, in the pages of the *Journal of Anatomy and Physiology*.

Starting from a solitary observation of the ligamentum teres in the ostrich chick, where he found it continuous with the pectineus muscle, Mr. Sutton arrives at the conclusion that *all ligaments are muscles which have undergone retrograde metamorphosis*, and this work contains the evidence he has since been able to collect in support of that conclusion. We are quite satisfied as to the accuracy, interest, and even importance of his facts, but we care little for his theory; and we believe that he must be aware that the evidence he gives as to the nature of the osseous spicule in front of the ankle-joint of birds tells much more against his theory than for it.

His observations on the nature and arrangement of the gleno-humeral ligament are highly important. He entirely confirms the view that this ligament is the representative of the ligamentum teres in the hip, and sums up in the following paragraphs:—

"1. Every mammal in whom I have, up to the present time, found a gleno-humeral ligament, likewise possesses a ligamentum teres in the hip.

"2. Those mammals in whom a ligamentum teres is absent, also lack a gleno-humeral band.

"3. In those forms in whom the gleno-humeral is well developed, the ligamentum teres of the hip is also very thick and strong."

These conclusions are based upon the dissection of over 250 animals, belonging to 150 species—figures which give a striking testimony to Mr. Sutton's industry, as well as some notion of the unrivalled opportunities he has for morphological investigation.

The book is full of interesting and suggestive facts, but as most of them are "caviare to the general," we refrain from discussing them in these pages.

In a work of this nature, elegant English is not to be expected, but the author might at least have spared us the grotesque sentence with which he ushers in his first chapter.

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*Treatment of Disease in Children, including the Outlines of Diagnosis and the Chief Pathological Differences between Children and Adults.* By ANGEL MONEY, M.D. London: H. K. Lewis. 1887.

THIS book is written in such a loose, jerky, pseudo-epigrammatic style as to considerably detract from its value. As its title implies, the author has naturally found it impossible to confine himself to the treatment of disease. He has, therefore, added brief descriptions of the main symptoms and of the pathology of the various diseases, and in these his unfortunate style frequently renders his meaning obscure. The greater portion of the book is devoted to treatment, and the practitioner or student will here find a tolerably good *résumé* of what has been recommended in each disease. The author gives prominence, of course, to the treatment which he himself approves of.

A few extracts may be given to illustrate what has been said above. In the chapter on "General Treatment in Disease" we read: "Nature requires modification as civilisation advances from age to age. It is heard not unfrequently that treatment by drugs is unnatural. 'It cannot be good to take so much medicine' is a vulgar cry. Now, what is Nature? Nature is organised accident. She is stereotyped art. But we must

not forget the artificial foundation of Nature." Such a paragraph requires no comment. Again, among the directions for applying a wet pack, the following extraordinary one is given (p. 487): "The pack should remain about fifteen minutes or less in an infant." Poor infant! This may be a slip, but on page 490 the author, by inference, commits a grave mistake when he says: "Stimulants are not always needed in the treatment of fevers in children." Surely Dr. Money cannot mean that in the majority of cases they are required.

This book may be used for reference when one wishes to see what treatment has been given in any special disease, but it cannot replace any of the text-books on children's diseases now before the profession, and it does not even supplement such a work as Eustace Smith's to any notable extent.

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*On Overwork and Premature Mental Decay: its Treatment.*

By C. H. F. ROUTH, M.D. London: Baillière, Tindall, & Cox. 1886.

DR. ROUTH is a pessimist. The struggle for existence seems to him to be causing a rapid degeneration of the race, and the picture he paints is, therefore, by no means an agreeable one. Fortunately his broad statements are not based upon an assured foundation; his data are not satisfactory, to us at least. There is no doubt that many breakdowns occur from continued worry, anxiety, working at high pressure, functional excesses, &c.; but the extent of the evil is exaggerated in this essay.

In regard to both the causation of mental decay and its treatment, Dr. Routh lays great stress on an analysis of brain tissue by Thudichum, a chemist who has among many the reputation of being highly speculative. The result is that phosphorus is set forth as the most important constituent of brain matter, and hence as the most serviceable remedial agent, whether in the form of pure phosphorus or in the form of fish. The argument in its favour does not seem much superior in merit to that met with in the old story, according to which a lady was advised to feed her marriageable daughters on fish, because fish contained phosphorus, and phosphorus was good for making matches.

We cannot commend this little book to our readers as containing anything novel. It looks as if meant for the laity rather than for the profession.

*A Dictionary of Terms used in Medicine and the Collateral Sciences*, By the late RICHARD D. HOBLYN, M.A., Oxon. Eleventh edition, revised throughout, with numerous additions. By JOHN A. P. PRICE, B.A., M.D., Oxon. London: Whittaker & Co. 1887.

THIS dictionary is more than a mere vocabulary of terms. It gives the terms, their derivations, and a brief account of their meaning and use. This will appear from an example taken at random :—

“*FILARIA SANGUINIS HOMINIS*. An embryonic nematoid worm found in the blood, lymphatics, and kidneys of man. Manson believes that the ova by obstructing the flow of lymph are the cause of lymph scrotum and chyluria.”

We are able to say from several experiments that the work is on the whole well done. The descriptions are necessarily brief, but accurate.

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*Lehrbuch der Physikalischen Untersuchungs-methoden Innerer Krankheiten*. Von DR. HERMANN EICHHORST. Zweite Auflage. Braunschweig: Wreden. 1886.

IN the February number of this *Journal* for 1882 we reviewed at length the first edition of this work. It has now reached a second edition, thus confirming the high opinion of it we then expressed. Rewritten in great part, and considerably enlarged, with the addition of a chapter on the Nervous System, it is one of the most complete works on the subject, and well up to date. To the already numerous list of illustrations 79 have been added, all of them reflecting credit on the engraver. We would be glad to see this work in an English version, so as to put it within the reach of a larger circle of readers.

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*Thomson's Conspectus, adapted to the British Pharmacopœia of 1885*. Edited by NESTOR TIRARD, M.D. London: Longmans, Green & Co. 1885.

THIS is a useful condensation of the pharmacopœia, in which much of the descriptive detail found in that volume is omitted. It is handy in form, well got up, and apparently accurate, and is furnished with the usual appendices on new drugs (apparently indispensable now in any work on *materia medica*), on poisons, the art of prescribing, &c.



*The Year Book of Treatment for 1886.* London: Cassell & Company. 1887.

THIS very satisfactory therapeutical annual continues to fulfil efficiently the purpose for which it was started—namely, to provide both an account of the more recent advances in treatment and a review of these advances by competent authorities. Few points of any importance seem to have escaped the notice of the various editors, and reference to the very varied contents of the volume is rendered easy by copious indices of author's names and subjects.

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*Massage as a Mode of Treatment.* By WILLIAM MURRELL, M.D., M.R.C.P. Third edition. London: H. K. Lewis. 1887.

THE appearance of the third edition of this work within nine months of the appearance of the first is proof sufficient of its success, and of its having supplied a demand. As we have already noticed at some length the previous edition, it is unnecessary to do more than call the attention of our readers to the publication of the third, and to say that it has been considerably enlarged.

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## MEETINGS OF SOCIETIES.

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### GLASGOW OBSTETRICAL AND GYNÆCOLOGICAL SOCIETY.

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SESSION 1886–87.

MEETING VIII.—25TH MAY, 1887.

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#### I.—NOTES OF DIFFICULTIES OCCURRING IN ELEVEN YEARS' PRIVATE OBSTETRIC PRACTICE.

BY DR. ROBERT POLLOK.

A small pediculated polypus which he had removed from the os uteri of a patient who had been wearing a pessary for prolapse was exhibited by Dr. Pollok, after which he proceeded to read a paper entitled "Notes of Difficulties occurring in Eleven Years' Private Obstetric Practice" (2,000 cases),

with treatment adopted. After a few preliminary remarks, he said his most frequent troubles in early practice had been placenta adhærens and hour-glass contraction. These, he now considered, must have arisen from awkwardness, and lack of firmness on his own part, from the fact, that as courage strengthened and experience lengthened, such cases became entirely eliminated. That hour-glass contraction might occur in the hands of the most experienced was not to be gainsaid, but "it was so easily overcome by the steady and bold insinuation of the hand in the form of a cone through and beyond the constriction, simultaneously subduing the spasm and obtaining the expulsion of the placenta," that he no longer classed it as a marked difficulty.

Apart from cases of retained placenta from uterine inertia or irregular contraction, he had only met with three cases of true morbid retention due to "adhesion of the membranes to the uterine wall to such an extent that their entire removal was an impossibility." He removed as much as possible, "passing the hand to the fundus, and peeling from above downwards." They all ended favourably. Hæmorrhage was carefully watched, and "antiseptic uterine injections were diligently applied for the first week," the solution used having been a tepid one of carbolic acid and water (1 to 80). He thought that Koch's solution (1 to 3,000), so beneficial surgically, produced much irritation and erythema, and he had therefore discontinued its use in obstetric practice. The douche was preferable to the syringe for all cases of intra-uterine injection. It was necessary that in any case the os should be patent. He had once seen an acute metritis and cellulitis ending in abscess set up by intra-uterine injection.

In abortions, in contracted os, at first he had experienced the greatest difficulties with the membranes. In one case, the placenta had been retained three weeks. Notwithstanding that he was assured that "everything had come away," he decided to explore as the sound revealed subinvolution, and the hæmorrhage was intermitting and profuse. A tent was introduced, and the vagina plugged, on removal of which—24 hours afterwards—the os was dilated, and a putrid placenta got away. The case did well. He thought much time was often wasted in vain attempts to prevent abortion. Whenever there is pain, occurring rhythmically, and accompanied with hæmorrhage, he had never been able to prevent abortion, and when these phenomena now appear, he no longer attempts prevention. He did not think viburnum prunifolium of any avail in such cases. His plan was to plug the vagina at once

and administer 20 minims liq. secalis ammon. (Richardson) along with the same quantity of acid sulph. aromat. On the removal of the plug after an interval of 18 to 24 hours, it is rare *not* to find the ovum and membranes entire in the vagina. A convenient method of plugging is by means of a handkerchief petticoated to form an infundibulum in the vagina, into which the tampon material is inserted. The borders of the handkerchief hanging out form ends, traction on which facilitates the removal of the whole.

Very early in practice, he had been called to deal with a case of transverse presentation, where the membranes were already ruptured and the hand protruding beyond the vulva. Under chloroform he effected podalic version with great difficulty, owing to jamming of the shoulder. Death ensued from metritis on the fifth day. In a second case, though a smart metritis followed, the patient ultimately recovered, and was delivered of twins twelve months afterwards. In other five cases, where he was called to deal with transverse presentations prior to the rupture of the membranes, the results were favourable to all the mothers and infants. The usual examination having failed to reveal any presenting part, except, perhaps, a protruding bag of waters, there are grounds for suspicion sufficient to justify preparation for action, if need be. With the left arm bared, the os is gradually dilated with the fingers until a satisfactory exploration has been made. If a hand, shoulder, or thorax be diagnosed the whole hand is gently insinuated and carried to the fundus, the membranes ruptured, the foot seized and at once brought down, the cephalic portion being assisted in its ascent by the right hand on the abdomen. The left arm is never withdrawn, and the exploratory hand becomes the delivering one if necessary. With such ease had he effected this that the mother and friends were not even aware of aught having been wrong, a consideration which immensely affected the mother for good. He did not leave a single step of such cases to Nature. She had blundered egregiously, and is not to be relied on in the circumstances. The plentiful secretion of liq. amnii should be taken immediate advantage of. In books directions are given to diagnose whether the position be dorso-anterior or dorso-posterior, and the direction also given to bring down the breach and wait. He thought anxiety to be precise in diagnosis might lead to the rupture of the membranes, and delay in doubt might lead to the same accident occurring naturally, and the case thus become exceedingly unfavourable both for mother and child.

He might be asked, Why not adopt the bipolar method? He did so, in a modified way—namely, by assisting the evolution externally, but not by Hicks' method of pushing up the presenting part with the fingers in the vagina. He thought it was impossible to reach the shoulder, in transverse cases, with the fingers, as Hicks recommended, without the greatest danger of rupturing the membranes, and thereby eliminating the mobility of the child. He had never found such marked difficulty in digital dilatation that some have represented. He had always found Barnes' bags unserviceable when required, and much inferior to the fingers. Neither had he experienced any of the dangers said to be due to introduction of the whole hand into the uterus, and believed that there was none where the membranes were unruptured.

The application of the forceps appeared with increasing percentage in direct ratio to years of experience, ranging from 9 per cent at first to 16 in his last year. This he believed to be due to three factors—viz., increased familiarity and faith in the instrument, decreased staying power of the patients, and a greater number of primiparæ at an advanced age. The forceps he used preferentially were Leishman's—a pair intermediate between the long and the short. For cases of actual obstruction at the brim, however, he chose the long curved forceps. He had seen injury result to both mother and child from attempts to drag the head through a contracted brim with straight instruments. Even with the curved instrument injuries will arise, and it is in such cases that his greatest mortality had occurred. He had used long forceps thirty-two times and had a maternal mortality of three; and on reviewing all the circumstances in connection with these deaths, he was of opinion that perforation ought to have been performed, and yet in one in which this was resorted to death also ensued. The conjugate diameter in these three cases was, as nearly as possible, three inches.

He did not think that turning should be resorted to after failure to deliver by forceps, and narrated a case where, in a conjugate of two inches, after craniotomy, the forceps were tried and thereafter the cephalotribe, but without effecting delivery. In this dilemma, Dr. Stirton turned and delivered by sheer force, the whole perineum being ploughed up and converted into a cloaca. The patient succumbed in two days. Cæsarian section might possibly have been better in this case, or the manœuvre adopted by Dr. Osborn in the case of Elizabeth Sherwood—viz., that of canting the base of the skull.

He had twenty-one breech cases, or 1 per cent. In such

he facilitated labour in every possible safe manner, but had never used any other instrument than the fingers. In two hydrocephalic cases he punctured with the small trocar and applied forceps. Both mothers recovered. In a case of general foetal anasarca, several breaches of continuity were made in the tissues, the fluid drained off, and delivery accomplished—the mother doing well. Two funis cases were rapidly delivered by forceps, one child living and the other being already dead for some time *ante-partum*. One anencephalic case gave no further difficulty beyond diagnosis and the large quantity of liq. amnii present. One brow and two face presentations were dealt with by forceps. A cystic tumour in the vagina proved a difficulty in one case. It burst during parturition, and the mother died from septicæmia, in spite of antiseptic precautions. In two cases of *ante-partum* hæmorrhage, due to injury, the os was dilated and forceps applied. Four cases of placenta prævia issued in two foetal, but no maternal deaths. In each case turning was performed. He looked upon immediate evacuation of the uterus as imperative in such cases, no matter at what period of pregnancy they come under observation.

Premature labour was induced three times, in view of impossible or dangerous delivery. In one case, the patient was small and deformed, and had a conjugate of two and a half inches, and the seventh month was chosen for the operation. The second was a woman of thirty-five years, who at former pregnancies had been delivered by craniotomy. The pelvis was of rachitic type, and measured under three inches. The end of the seventh month was the time chosen in this case also, and the result in both instances was satisfactory in respect of both mother and child.

In the third case the operation was rendered necessary by accidental hæmorrhage, repeated attacks of which threatened her existence. The foetus was dead. His method of proceeding consisted in the introduction of an elastic bougie or catheter into the uterus, the remaining portion being coiled up in the vagina. He then plugged, and in about twelve hours, on removing the tampon, the os will be found either dilated or dilatable. If not dilated, digital manipulation will soon effect that end and complete the labour. He was thoroughly convinced that the induction of premature labour should be largely extended, as he felt sure by this means the greatest safety was to be found for mother and infant in cases where the pelvis measured under three inches. If all suspicious cases were examined at date of engagement by the

medical attendant, and this plan adopted in suitable cases, less would be heard of the horrors of craniotomy and its surgical congener, Cæsarian section.

Paralysis of the lower extremities constituted a difficulty in one case. Chloroform and forceps were used, and, though there was uterine inertia, and the whole labour had to be assisted, the mother and child did well.

Of three cases of *post-partum* hæmorrhage one was fatal. It occurred in an unmarried woman who was a "bleeder," though this was not known to him till afterwards. She was thoroughly broken down, anæmic, and dejected with grief at her unfortunate position for the second time. Slight hæmorrhage occurred prior to labour, and it recurred severely an hour afterwards, and before help could arrive she was moribund.

His invariable practice was to administer  $\mathfrak{z}\text{i}$  liq. secalis ammon. (Richardson) at the end of labour, just as the head is on the perineum—not with any view of expediting delivery (for he believed ergot was an uncertain and uncontrollable motor force, and that we had a much safer and more reliable aid in the forceps), but as an excitant to uterine contraction and involution, it was invaluable. In the case related ergot had not been administered. It was as a prophylactic he thought it was most useful. For the treatment of actual hæmorrhage he thought it a questionable remedy, and relied more on kneading, pressure, and alcohol. He thought that ergot had a depressing action on the heart, and, in the collapse of hæmorrhage, might extinguish the fluttering and feeble beat of the latter. He had had three cases of puerperal eclampsia, and had seen three cases outwith his own practice. The first was a plethoric case, and the seizures began one hour *post-partum*. The patient never recovered consciousness, being comatose between the attacks, which occurred at intervals of ten minutes. He was withheld from venesection by the consultant, who would go no further than the recommendation of a few leeches to the nape of the neck. Chloroform was administered, as were also chloral and potassium bromide, but with no permanent effect. The second case was one of twins. In this the convulsions also began after delivery. He did not wait for a consultation, but at once withdrew 8 ounces from the median basilic vein. The convulsions ceased immediately, and their recurrence was prevented by the administration of chloral. The third case was treated similarly, and both recovered. The fourth patient was anæmic, and therefore chloroform and nitrate of pilocarpine were the remedies employed, the latter being given with benzoic acid after consciousness

returned. She also recovered. In the fifth case—seen with Dr. Thomas F. Gilmour—the convulsions preceded labour. Chloroform was administered and delivery hastened. Bleeding was also resorted to, but the patient succumbed in about twelve hours. There was complete suppression of urine, only a teaspoonful being obtained by the catheter. The administration of pilocarpine was overlooked. Other two cases occurred after delivery, but the seizures were more epileptiform in type, and under chloral and bromides the convulsions disappeared.

He had had several cases of albuminuria with œdema. He treated them with bitartrate of potash and benzoic acid, with the result of recovery in all the cases, two being prematurely delivered. It was a question whether the induction of premature labour was not the safest course to adopt in such cases, bearing in mind that the slighter the œdema the graver was the case.

Extra-uterine pregnancy had occurred once. Mrs. C. had been attended in previous confinements by him, and on this occasion complained of a severe pain in the right ovarian region. She reckoned herself to be three months pregnant, and was afraid she had sustained some injury in the execution of her household duties. In conjunction with Dr. Hugh Kelly he treated the case at first as one of localised peritonitis, by means of belladonna externally and opium internally. He was afraid abortion would occur. The pain, however, gradually subsided, and the patient seemed to be recovering, when hæmorrhage occurred. In conformity with his usual practice he plugged, and looked for the onset of labour. On removing the tampon next day a piece of decidua (placental in structure) was obtained, and the plug was reintroduced. The hæmorrhage ceased, and the second tampon was removed in fourteen hours, but there were no further signs of abortion. The sound was then introduced, when the uterus was found to be of normal dimensions, but the abdominal enlargement was not at all reduced. He was suspicious of the nature of the case, and considered that the history, palpation, and abdominal ballottement revealed pregnancy. Dr. W. L. Reid then saw the case, and agreed that the decidua was placental, and advised expectancy. After three weeks it became evident, from the presence of a low fever and the odour of her breath, that she was lapsing into a condition of sepsis. She was becoming exhausted, and expectancy was no longer admissible. She was removed then to the Training Home for Nurses, and a consultation was again held, at which Drs. Reid, Hector Cameron, and Finlayson were present. Abdominal section was decided upon, and the

operation was performed by Dr. Reid, assisted by Dr. Cameron. A cyst was found containing putrilage possessing a most abominable odour. Perhaps this might have revealed shreds of disintegrated foetal tissues, but no foetus was found. It was concluded that the decomposition of the foetus and its solution gave rise to the putrescence, death of the foetus probably taking place when the placental mass was thrown off a month previously. The cyst was stitched to the abdominal wall and sloughed away. The woman made an excellent recovery.

*Dr. W. L. Reid* said he thought the irritation caused by mercuric chloride solution, as mentioned by Dr. Pollok, must have been accidental. He thought it was a risky proceeding to leave a plug in twenty-four hours, especially if only soaked in 1 to 80 carbolic solution. He thought twelve hours quite long enough. Neither was he satisfied of the total absence of danger from manual dilatation. He thought it was always a good plan also to fill the vagina with a carbolised fluid first. Bougies and catheters—the latter specially—were to be used with exceeding care. He thought a pump action was inseparable from the use of a catheter—one case where it was used within his knowledge having proved fatal. He regarded a solid instrument essential.

The diagnosis of the extra-uterine pregnancy was hampered by the fact of gas having been generated in the interior of the cyst. The portion of the cyst, as elicited by the section, was superior to the Fallopian tube. Bronzed skin and mummyish tissues were features of the case, but these phenomena rapidly disappeared after the operation.

*Dr. Murdoch Cameron* condemned the use of straight forceps, but if straight, he thought the smaller they were the better. Simpson's curved he thought suitable for use either at the brim or outlet, and he only used the one instrument. His experience of precipitate labour differed much from that of Dr. Pollok, as he had had too many cases of the kind, and the narration of some of these caused much amusement. He had seen bronzing and mummyish tissues without extra-uterine pregnancy. He laid great stress on cleanliness, but contented himself with soap and water.

*Dr. Abraham Wallace* had used a catheter seven or eight times, in the manner described, for induction of premature labour, and had never had any trouble from doing so. He advocated the use of Barnes' bags, however, and felt certain there were many cases where manual dilatation was impossible. He thought there was not much benefit to be derived from "slumping" cases, and that more profit was derivable from



detailed study. He questioned the diagnosis in the case of supposed extra-uterine pregnancy. The amenorrhœa and abdominal ballottement were not sufficient to justify it.

*Mr. Nairne* here interpolated that he had seen the case and failed to make out ballottement, and *Dr. W. L. Reid* said he certainly did not think it was an ordinary dermoid cyst or abscess.

*Dr. Wallace*, continuing, remarked that with reference to the left hand, it was very useful to put the patient in the genu-pectoral position and inject an antiseptic fluid. Revolution was more easily accomplished thus. It was better to use both hands, however, or either according to the position of the foetus. He advocated the use of Simpson's forceps, and used tapes or cords for traction instead of Tarnier's cumbrous instrument.

*Mr. Thomas F. Gilmour* explained, in reference to the eclamptic case alluded to, that it was an uræmic one, and with regard to the case which had been so long in labour, it was an exceedingly narrow pelvis. One hundred and thirteen hours elapsed betwixt the breaking of the membranes and delivery. The case illustrated what Nature could do by simple waiting.

*Dr. Tindal*, referring to the treatment of abortion cases, asked if it was the case, that when anything was retained in the uterus the os was patent? He was under that impression. He asked what *Dr. Pollok* would do in a case like this:—A lady out walking is suddenly seized with severe hæmorrhage. She faints and becomes almost pulseless. She is plugged. Ergotine and brandy are administered; she rallies. Plugs removed; nothing expelled; os closed. Tampon reinserted; after 12 hours no result. Tamponed again, and again no result. Effort made to dilate; failure. Consultation; expectancy advised. Ergot administered; days elapsed; no discharge; recovery so far. Consultation again; expectancy advised. At the end of 3 months, spontaneous expulsion, without hæmorrhage, of an ovum and placenta. He had had experience of precipitate labours, but it was seldom aught serious resulted. In one primipara, however, he found the os closed, barely admitting his finger tips. He advised patience, and that everything would go well. He hardly got home, however, when he was rung up, and refused to go back with the messenger. *Mr. Nairne* was then sent for, and when he arrived the child was already born, and the perineum was ruptured to the third degree.

*Dr. Miller* said he never used straight forceps at all. He thought Barnes' were applicable to all cases. He could not go

all the way with Dr. Pollok in regard to the induction of premature labours, and the treatment of abortions. He always advised rest, and opium in the first instance at least, and so with placenta prævia. He objected to examination of patients at the time of engagements. He preferred the bougie to the catheter in the induction of labour, and obtained results in from 6 to 12 hours.

*Dr. G. A. Turner*, referring to the use of viburnum prunifolium, stated that he had treated successfully 6 cases of threatened abortion with it. They all were characterised by bleeding—in two it was sharp—and rhythmic contractions also in two cases. As to the comparative merits of the bougie and catheter, he had used a catheter filled with carbolic glycerine to induce labour in a case of continued hæmorrhage at the 4th month, and with success in three hours; and in a second case later in pregnancy within 7 hours and a living child. Nevertheless, he thought there was force in what Dr. Reid stated, and he would not use a catheter again.

*Dr. Sloan* stated that early phthisis was sometimes arrested and sometimes cured by pregnancy. He objected to plugs being left in for more than a few hours, and advocated mercuric chloride as an antiseptic. His experience was not favourable to viburnum, and he deprecated the use of a handkerchief, and advocated the use of a kite tail tampon for plugging. He remarked the absence of any allusion to cephalic version in the paper read. He considered the fingers almost useless when the breach was jammed, and said that a copper tape had been found very useful in these cases. He thought immediate treatment called for in cases of placenta prævia. Premature labour he always delayed till the end of the 8th month. He employed a bougie or a catheter indifferently, and his results were about equal with either. He had observed labour to take place very satisfactorily in cases of paraplegia. He objected to ergot being given prior to the passing of the perineum, and he thought it was useless when administered by the mouth *post-partum*. As to the case of supposed tubal pregnancy, he thought Dr. Pollok must have made a mistake in thinking that he had felt ballotement.

*Dr. Pollok*, in reply, said he did not fear sepsis as much as some seemed to do, considering how frequently vaginæ were in a septic condition. Neither did he restrict himself to one hand in turning, but mentioned his cases as those where he had concluded his operation without withdrawing his hand. He held that even 2 oz. of hæmorrhage justified immediate interference if accompanied by rhythmic pains and patent os.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

### NERVOUS DISEASES.

By DR. G. S. MIDDLETON.

**Infantile Paralysis of Cerebral Origin.**—Spastic hemiplegia of infants, as described by Ross and others, follows an illness characterised by convulsions and unconsciousness, from which latter the child recovers hemiplegic. Subsequently contractures set in; athetosis and choreic movements may supervene at the tenth or twelfth year, epileptic attacks occur, and the intellectual faculties fail. The lesion is a diffuse sclerosis with retraction of the affected area in the motor region of the cortex, and among the possible causes of the primary lesion Ross suggested a local encephalitis.

Strümpell, in the *Deutsche Medicinische Wochenschrift*, x, 44, described a polio-encephalitis acuta, founded on the observation of 24 cases; and Ranke has more recently, in the *Jahrbuch für Kinderheilkunde*, xxiv, 1 and 2, contributed a further series of 9 cases from which a tolerably accurate picture of what seems to be the early stage of the spastic hemiplegia of Ross is drawn. These cases occurred at ages varying from 6 weeks to 6 years. In the majority no special cause was apparent. One followed measles, and another scarlet fever. But, as in polio-myelitis, healthy children seem most frequently attacked. There is usually an initial stage of fever, with vomiting, convulsions, and sometimes unconsciousness; but there may be no premonitory symptoms. After two or three days the child is noticed to have lost power over muscles on one side of the body. There is more often total paralysis of arm than of leg; and facial paralysis is seldom extensive or of long duration. Sometimes there is no paralysis, but an ataxia. The affected muscles do not undergo a degenerative atrophy, nor do they present the reaction of degeneration. Their normal tone may be preserved; the flaccid condition of polio-myelitis is not observed. The reflexes are not diminished, but may be increased; and sensation is generally normal. Ultimately, as a result of the cerebral irritation there may be epileptic attacks, hemichorea, athetosis, and contractures, and there may be affections of speech if the left hemisphere is involved. Strümpell draws a parallel between the cerebral and spinal affection. In both, healthy children are attacked after an initial period of fever, and in both, the grey matter is the seat of the disease, the anterior cornua of the cord being affected in polio-myelitis, and the motor area of the cerebral cortex in polio-encephalitis.—(*Medical News*, 16th January, 1886.)—A. K. C.

**A Cure for Neuralgia.**—Dr. Henry G. Davis (*Boston Med. and Surg. Journal*, 18th November, 1886) says, "It may be thought incredible, yet, nevertheless, it is true, that he has never seen a case of pure neuralgia that the following prescription would not cure:—

|                            |             |
|----------------------------|-------------|
| "℞. Tinct. cinchonæ comp., | 5ij.        |
| Tinct. nucis vom.,         | 5ss.        |
| Morphiæ sulph.,            | grs. ij. M. |

Sig. One teaspoonful every three hours."

D. M'P.

**Pseudo-Tabes from Arsenical Poisoning.**—Dr. Dana records two cases in which the symptoms closely resembled those of *tabes dorsalis*, but were due to arsenic. In the first case, the patient had been put upon Fowler's solution, in gradually increasing doses, and during the whole of the fourth month of treatment was taking half an ounce thrice daily. The symptoms affected both arms and legs, and were recovered from only five months after the

arsenic had been discontinued. The second case was that of a man who had swallowed a handful of Paris green, the sensory and atactic symptoms, which in this case also affected both arms and legs, setting in six days later. The patient improved very slowly. In both cases, among the early and most troublesome symptoms were burning sensations in the limbs. Dr. Dana gives a bibliography of the subject, and discusses its pathology. He believes "that arsenical paralyses, like those from diphtheria, alcohol, lead, and probably other infections and poisons, are of two types:—

(a.) The ordinary mixed motor and sensory paralysis; the motor troubles and atrophy being more marked.

(b.) The pseudo-tabetic form in which there is no pronounced motor paralysis, but marked sensory troubles, and especially ataxia."

In the absence of *post-mortem* examinations, he reasons from the analogy of alcoholic pseudo-tabes that the arsenical form is due to a peripheral neuritis, and not to a myelitis.—(*Brain*, January, 1887).

**The Action of Sulphuric Ether on the Peripheral Nervous System.**—Dr. H. P. Bowditch, in the April number of *The American Journal of the Medical Sciences*, summarises as follows the principal results of a series of researches upon this subject which were carried on under his direction in the Physiological Laboratory of Harvard University:—

1. The ordinary effect of electrical stimulation of the recurrent laryngeal nerves of dogs is to cause constriction of the glottis, but if the animal is thoroughly etherised dilatation may be produced.

2. If the animal is partially etherised, the effect of the stimulation will vary with the strength of the current, a weak irritation causing dilatation, and a stronger irritation causing constriction of the glottis.

3. The more complete the etherisation the greater is the intensity of the current necessary to produce constriction.

4. A similar "ether effect" may be observed when the sciatic nerve of the frog is stimulated, the ordinary extension of the leg and abduction of the toes giving place, under the influence of this drug, to flexion and adduction. It is, however, always possible to produce the ordinary effects by increasing the intensity of the stimulation; and, on the other hand, it is possible, by using very feeble stimuli, to produce without etherisation the same effects which occur on the application of stronger stimuli to animals under the influence of the drug.

5. This "ether effect" on frogs may be observed, both when the ether solution is introduced by cutaneous absorption, and when it is applied locally to the nerve below the point of stimulation. Hence the most reasonable explanation of the phenomenon is that the drug, by diminishing the conducting power of the nerve, causes a strong irritation applied to the nerve to become a weak irritation when it reaches the muscles.

6. This explanation cannot be applied to the phenomenon as exhibited by the recurrent laryngeal nerve of the dog, for without ether feeble irritations do not produce dilatation, and, with complete etherisation, strong stimuli do not produce constriction of the glottis.

**The Pupil in its Semeiological Aspect.**—Many observations have been made, from numerous standpoints, regarding pupillary conditions, yet with a few notable exceptions they have been studied in an isolated manner, relative to the particular disease or lesion of which they might be more or less symptomatic. They have been often looked upon, and are still regarded by some, as curious, interesting, but erratic phenomena, far too variable to be depended upon, and without any connecting thread upon which these conditions, as seen in a variety of diseases, could be strung. Few attempts have been made to grasp pupillary manifestations as a whole, still fewer to reduce the varying phenomena to principles, or to reduce the laws by which they are controlled.

In an interesting paper in the July number of *The American Journal of the*

*Medical Sciences.* Dr. William Macewen, of Glasgow, gives a brief outline of the physiological phenomena pertaining to the movements of the pupil, and then presents a series of personal observations.

He cites evidence to show that the suspension or abolition of cerebral function in the living body is attended by mydriasis, the latter being the sequent of the former. If inquiry be made concerning the mechanism inducing this pupillary effect coincident with the arrest of cerebral function, the theory which explains the greater part, if not the whole of the phenomena, is that which has been so ably advocated by Mosso. The passive movements of the pupil are regulated by the vascular system of the iris, which is in complete harmony with that of the encephalon. In these conditions inducing general suspension of the cerebral function, a state of ischæmia prevails in the brain and iris inducing mydriasis. This likewise obtains in unilateral lesions, where the pressure is so great as to induce anæmia of brain and iris. Myosis may also be brought about by a like mechanism acting in the opposite direction. The "irritation" setting up congestion of the cerebral and meningeal vessels, leads to congestion of the vessels of the iris, and so produces contraction of the pupil.

Dr. Macewen shows that when the function of the brain is in abeyance, the pupils are in a state of stabile mydriasis.

When the function of the brain is interfered with by conditions usually included under the term "irritation," the pupils are in a state of myosis—sometimes labile, but generally stabile myosis.

The same pathological factors which cause myosis may also cause mydriasis, the degree in which these factors are present being the determining point between the former and the latter, and not merely the particular locus in the brain.

When the function of one-half of the cerebrum is placed in abeyance by a superficial or cortical lesion, the pupil on the same side as the lesion is in a state of stabile mydriasis.

When the function of one-half of the cerebrum is interfered with by some source of cortical irritation, the pupil on the corresponding side to the lesion is in a state of myosis.

Hæmorrhage into the pons Varolii, when strong, causes strongly contracted pupils; but when it is more extensive, involving the grey matter beneath the aqueduct of Sylvius, a state of stabile mydriasis is induced.

Dr. Macewen concludes by pointing out the conditions under which myosis and mydriasis occur.

**The Cause of Contractures in Lateral Sclerosis.**—The following is an abstract of an editorial article in the *Boston Med. and Surg. Journal* for 31st March, 1887:—

Vulpian, in a recent work (*Maladies de la Moelle*, Paris, 1887), discusses the cause of the contractures met with in cases of lateral sclerosis of the spinal cord, whether primary or secondary, as well as in cases of multiple sclerosis and posterior sclerosis where the lateral columns are secondarily implicated. His conclusions may be thus summarised:—

1. The contracture is not due to *destruction* of the lateral columns, because experiments prove that complete destruction of the tract in an animal is followed by permanent paralysis of the limb, with permanent flaccidity.

2. The contracture is due to *irritation*. Experiments have shown that superficial cauterisation of the tract is followed by contracture of the limb on the same side. If the cauterisation were carelessly done, so as to destroy the tract completely, paralysis, not contracture, was produced; even in successful experiments the contracture was usually followed in some days by flaccid paralysis, owing to the destructive reaction caused by the cauterisation. Another proof is that when destruction was complete the limb became paralysed immediately: while contractures, in the successful cases, only came on gradually—in two or three hours.

3. The contractures are not the direct results of irritation of the motor

fibres of the lateral columns themselves, but are reflex. In many cases there has been sclerosis of the form under consideration, and in some cases the upper limbs have been "contracted" and the lower not, though *post-mortem* examination showed that the disease had descended below the level of origin of the nerves of the lower limbs. Careful consideration of cases leads to the conclusion that "In cases where the irritation of the lateral columns propagates itself to the excito-motor or sensory element of the posterior horns, the persistent excitation of these elements provokes the continuous activity of certain groups of cells in the anterior horns, and thus gives rise to tonic contractions of the muscles, which are supplied by nerves taking their origin in these cells.

"This physiological interpretation seems to harmonise with the results of the examinations of the medullary reflex functions in patients affected with lateral sclerosis, and in whom contractures tend to manifest themselves or already exist." That the reflex functions in these cases are exalted is shown by increased patellar tendon reflex, ankle clonus, and tremors so easily excited in other parts of the body.—D. M'P.

**Experimental Researches on Mercurial Poisoning : Lesions of the Peripheral Nerves.** By M. Letulle (*La France Médicale*, 11th January, 1887).—Partly from observation of cases of poisoning among workers in mercury, and partly from experiments on animals, M. Letulle concludes—(1) that mercurial differs from lead paralysis in the preservation of the normal electric contractility; in the absence of amyotrophy; and in the persistence of the tendon reflexes; and (2) that mercurial lesions of the peripheral nerves are characterised by the progressive destruction of the myeline with almost indefinite preservation of the axis cylinders, and without proliferation of the nuclei. These trophic alterations occur in segments around the axis cylinders. The condition of the cord and of the brain he reserves for further observation.

**Spontaneous Fractures in a Limb Suffering from Infantile Spinal Paralysis.**—(*La France Médicale*, 10th February, 1887).—Under this title, M. Bérbez records the case of a girl who, at three years of age, was seized with paralysis of the left arm, followed by atrophy, and presenting the usual characters of infantile paralysis. When about fifteen years old she struck her left arm against a ladder; the blow was insignificant, and yet the humerus was broken transversely below the surgical neck. The bone rapidly united, to again suffer fracture at the middle some five months later, as the result of a slight fall. Union took place rapidly, but the bone was again broken, just above the elbow, one day while she was in the act of putting her arm into the sleeve of her dress in dressing. M. Bérbez remarks on the rarity of trophic lesions of this sort in this disease.

**A Case of Recurring Attacks of Transient Aphasia and Right Hemiplegia.** By Edward D. Daly, M.D. (*Brain*, July, 1887).—A gentleman, aged 68, was suddenly seized, on 11th March, with aphasia and right hemiplegia, the attack lasting about five minutes, and passing off as rapidly as it came on. He had suffered occasionally for years from gout, most frequently affecting his bladder. The first sound of the heart was feeble; his arteries were thickened; his urine presented a little albumen. Temperature normal; pulse 80.

On 13th March, two similar attacks, each lasting about half an hour. On 14th March, ten attacks, the shortest lasting about ten minutes, the longest almost an hour. On 15th March, one attack of ten minutes' duration, and three in succession, with about a ten minutes' interval between, each extending over three hours. Since then there has been no attack.

There was never any convulsion. During the attacks the intelligence was certainly affected, but it was difficult to estimate to what extent. The aphasia was not complete; the hemiplegia was partial, and more marked in the arm than in the leg. There was slight anæsthesia of the right side.

The attacks always ceased abruptly; after the longer ones speech was recovered a few minutes before power in arm and leg. Dr. Daly is in doubt as to the pathology of this case, but suggests that it may have been due to spasm of the Sylvian artery, the rapidity of onset and of recovery apparently excluding thrombosis or other such lesion.

**Migraine attacks followed by Temporary Paralysis of the Third Nerve.** By C. W. Suckling, M.D. (*Brain*, July, 1887).—The patient, a youth of 18, had suffered since infancy from severe headaches, now increasing in frequency. "An attack lasts two days, and takes the following course:—He first feels a slight pain above the left brow, which gradually increases in intensity, and is followed by a flow of saliva into the mouth, and he feels very cold. He at once goes to bed on feeling the onset of an attack, but cannot sleep, and he has to remain in bed for two days without being able to sleep or to take any food. The pain increases gradually, and attains its maximum in twenty-four hours; it then as gradually subsides, and ends completely in another twenty-four hours. The pain remains localised over the left brow; he does not vomit, and there are no ocular spectra or paræsthesiæ of any kind, except that he complains of a nasty taste in his mouth. After an attack the scalp is tender." The affection of the eye was first noticed after an attack in infancy, and has frequently recurred, especially after severe attacks. "The left eyelid begins to drop at the end of an attack when the pain is wearing off. The lid takes twenty-four hours to drop completely, and in another twenty-four hours has usually recovered." The eye has twice also been turned outwards. Treatment by guarana in thirty grain doses, taken every hour at the commencement of the attack, has shortened the duration of the attacks to a few hours, and they have not been followed by paralysis. Dr. Suckling has found only one similar case recorded.

**A Case in which Paralysis of the Sphincters and Incontinence of Urine were, together with Torpid Intellect, the Chief Symptoms of Symmetrical Disease of the Corpora Striata.** By Jonathan Hutchinson, F.R.S. (*Brain*, July, 1887).—Advice was sought by a gentleman, aged 54, for incontinence of urine. Examination detected no considerable enlargement of the prostate, but very complete relaxation of the sphincter ani. He made no other complaint, and it was only from his friends that information was received of "utter mental prostration and physical inertia which seemed to be overtaking him." From being an austere and precise man, he had become effusive and jocular. He was constantly passing water as he sat in his office without seeming materially annoyed about it. His memory and intelligence appeared to be perfect, but he was rather too cheerful; there was no stage of elation such as occurs in general paralysis. There was no paralysis of motion, and no anæsthesia; the knee-jerk was good; eyes normal. He rapidly lost flesh and strength, and the mental torpor increased. While in this condition he suddenly sank into a state of coma, and died in a few hours. Examination revealed destructive disease (sarcoma) of the anterior and inner parts of the corpus striatum on both sides, with hæmorrhage on the right side only. Dr. Hutchinson notes specially the absence of limb paralysis, and considers it very possible that the symmetry of the lesion had been productive of bilateral weakness (of which there was elicited some account), and that thus the defect had been less easy of discovery. He refers to a somewhat similar case recorded by Bright in the second volume of his *Medical Reports*.

## MEDICINE AND PATHOLOGY.

By DR. JOHN LINDSAY STEVEN.

**On the Etiology of Movable Liver (Wanderleber).** By Dr. J. Kranold.—The author mentions that 17 cases of this rare affection have been recorded, and that the most probable cause is the development (perhaps congenital) of a hepatic mesentery. He then relates the case of a slenderly formed woman, aged 38, who had borne 11 children, and who had been confined 10 weeks before he saw her. Soon after her confinement she had the feeling as if there was something moving in the right side of her abdomen. On examination, in the right lumbar region was found a tumour having the characters of a uniformly enlarged liver, and not capable of being completely replaced. After a time, severe pain came on in this region, with great tenderness on pressure. On the surface of the liver rough, painful nodules could be felt, and the left lobe was uniformly enlarged, the anterior margin being rounded. Ascites was present. The tumour increased rapidly in size, and death took place in five weeks. The diagnosis was primary cancer of the liver. At the *post-mortem*, the first in a case of the kind so far as the author knew, it was found that the liver was completely separated from the diaphragm, that it was not by any portion of its parenchyma immediately in connection with the diaphragm, that the two layers of the peritoneum passing from the diaphragm were united and separated again to pass over the liver so that in reality a band quite analogous to the mesentery (*ein mesohepar*) was formed. For the diagnosis of "wander-liver," the following signs are of importance:—A tumour in the middle or lower abdomen agreeing with the liver in its physical characters; the absence of hepatic dullness at its normal situation in which an area of highly tympanitic percussion is found; the swelling movable and tolerably, if not entirely, capable of being replaced; the surface smooth. (*Deutsche Medicinal-Zeitung*. 1887. No. 56, page 628).

**Disorders of Digestion.**—The functional and organic diseases of the stomach have been much studied of late by our German confrères, but space forbids us to do much more than simply mention the titles of some of the papers that have recently appeared. In almost all the cases recorded the researches have been conducted by washing out the contents of the stomach and carefully testing them some hours after a test-meal has been taken. In this way the amount of free hydrochloric acid, and the peptic value of the gastric juice, are carefully estimated, and the relationship of the different states of the gastric secretion to morbid conditions of the mucous membrane is studied. The details of the different experiments must be obtained from the original papers themselves, and we would advise all our readers who are specially interested in diseases of the stomach to peruse them carefully. The following are the titles of some of the papers referred to:—

1. "Contributions to the Modern Methods of Diagnosing Diseases of the Stomach," by Fr. Krans. *Prager med. Wochenschrift*, Nos. 7-9, 1887.
2. "Observations on the Disappearance of Hydrochloric Acid Secretion and on the Course of Catarrhal Affections of the Stomach," by M. Jaworski, from the Clinique of Prof. Korczynski, of Cracow. *Münch. med. Wochenschrift*, Nos. 7-8, 1887.
3. "On Continual Secretion of Gastric Juice," by Prof. Franz Riegel. *D. med. Wochenschrift*, No. 29, 1887.
4. "On the Formation of Sugar in the Stomach and on Acid Dyspepsia," by Ewald. *Berl. Klin. Wochenschrift*, Nos. 48 and 49, 1886.
5. "Clinical Researches on Gastric Digestion in the Insane," by Carl von Noorden. *Arch. f. Psych.*, Bd. 18, H. 2, 1887.
6. "A Case of Complete Atrophy of the Gastric Mucous Membrane," by B. Lewy. *Berl. Klin. Wochenschrift*, No. 4, 1887.



To those interested we would specially recommend a careful perusal of papers 2 and 3 of this list, in which a clear and easily understood account is given of certain morbid conditions of the stomach directly related to disordered states of the gastric juice.—(*Deutsche Medicinal Zeitung*, No. 64, 11th August, 1887.)

**On the Presence in the Blood and Urine of Certain Diabetics of a New Substance, "Levogyre."** By Louis Hugoung. (*Lyon Med.*, No. 12, 1887.)—It has been shown that the quantity of sugar in the urine, as tested by reduction and polarisation, frequently varies. As the cause of this variation the author has found a substance in diabetic urine which he calls B-Oxybuttersäure (Lévoyre). This substance is a homologue of lactic acid (Fleischmilchsäure), and belongs to the higher series, containing 1 atom more of oxygen and 2 of hydrogen. It forms a thick, colourless, non-crystalline, strongly acid syrup, similar in reaction to lactic acid, and turns polarised light to the left. He has found it present in quantities of 4 in 1,000, whilst Kulz and others have obtained larger quantities. As grape sugar turns polarised light to the right, if both be present, the polariscope and the Fehling's test will give different results as to the amount of grape sugar present. One cannot, therefore, depend on the polariscope alone; and if such a difference in the two methods of estimation is found, then oxybutyric acid is present. Details of the methods of detection are next given, which are too complicated to be given here, and which must be turned up in the paper itself.—(*Deutsche Medicinal Zeitung*, No. 65, 15th August, 1887.)

**The Contagium of Scarlet Fever: A Critical Review.** By George Thin (*Brit. Med. Jour.*, 20th August, 1887).—This article consists of quite an exhaustive consideration of the evidence thus far brought forward regarding the specific poison of scarlatina.

The following criticisms are made upon Mr. Power's conclusion that the milk which produced the epidemic of scarlatina became infected from the disease upon the udders of the Hendon cows. Three herds became infected by sale and change of stock, the disease being in all respects identical, so far as one could judge from the descriptions.

The milk from *only one* dairy was associated with the epidemic. No contagion was carried from the other dairies. The result of inoculation of the milkers' hands was not scarlatina, but symptoms of local poisoning, with sometimes those of mild general septicæmia.

Dr. Klein found a streptococcus in the ulcers upon the udders. He inoculated calves, produced a general febrile disease—in one case fatal—and obtained the same coccus from the blood and organs. He found in the blood and organs of scarlatinal patients the same organism.

This looks certainly very plausible, but the writer's objection is that the finding of the same coccus in the two situations does not prove that it is the cause of scarlatina. There is no proof that the coccus has produced in man the disease scarlatina; and, secondly, there is wanting the proof that the disease which it has produced in animals by inoculation is scarlatina in any form.

The visceral lesions of scarlatina, even including the nephritis, are not sufficiently characteristic to enable us with certainty to distinguish it from other infectious disease or forms of blood-poisoning.

All the symptoms and all the lesions found in Dr. Klein's animals would be sufficiently explained by septic inflammation or blood-poisoning better even than by scarlatina. Nothing comparable to the eruption and desquamation of scarlet fever was ever produced in the calves, nor was ever seen in the Hendon cows.

After noticing the large number of writers who have observed micro-organisms of various kinds in scarlatina, the writer passes to the consideration of the "bacillus scarlatinæ" discovered by Edington (*vide Archives of Pediatrics* for July).

This bacillus was found in the blood in the early stages, but not late, and found in the desquamation in the later stages, but not early. It is the latter fact that gives these experiments their greatest weight.

Sufficient control experiments have not been made by Edington or others upon the bacilli of healthy skin and the skin in diseases other than scarlatina. These will doubtless soon be forthcoming.

The only absolute demonstration that Edington's bacillus is the cause of scarlatina will be that, when introduced into the blood of the human subjects, it produces the disease. Next to this in value will be that the organism is always found in the blood and organs of scarlet fever, but never elsewhere. We will be long waiting for such a position to be established. To be sure, Edington produced in animals a disease attended by erythema and followed by desquamation. This is not enough. There are many forms of erythema not scarlatinal which may even desquamate. We must see produced in animals a disease so strikingly like human scarlatina as to satisfy all clinical observers; or animals suffering from inoculation must communicate scarlatina to man. None of these criteria are as yet satisfied.

The work of Klein and Edington has opened the way; others must now step in and fill up the gaps before our knowledge on this topic can be complete.—(*Archives of Pediatrics*, September, 1887.)

**Angina Lacunaris and Diphtheritica.** Fränkel (*Jahrb. f. K.*, Nos. 17, 18, xxvi, 1; from *Berl. Kl. Wochen.*, 1886).—Angina lacunaris, or follicular angina, is an infectious disease. This is shown by the course of the fever which accompanies it, the existence of swelling of the spleen, &c., and it shares with other infectious diseases, such as erysipelas, articular rheumatism, and pneumonia, the peculiarity that it establishes a predisposition to such a disease. Angina lacunaris also sometimes occurs as an epidemic, and the fact that it may take the form of what might be called a family epidemic leaves little room for doubt as to its infectious character. The disease is constituted by an inflammation of the mucous membrane of the lacunæ or crypts in the tonsils; a muco-purulent secretion exuding from them upon the surface of those bodies. The result of this exudation is a tender, dirty-white deposit, which contains no fibrine. In exceptional cases the process extends so as to include the arch of the palate and the entire surface of the tonsils, but such cases are probably not genuine cases of angina lacunaris. The author's bacteriological investigations for the purpose of finding the medium by which the infectiveness of this disease is transmitted have not as yet been concluded. The fact that taking cold has a well known influence in exciting this disease does not efficiently oppose the possibility that a fungus may play a rôle in that direction. Taking cold might have the significance of an injury to the bones, which must precede the development of osteo-myelitis when this is to be produced (artificially, as in an animal?) by an injection of staphylococcus aureus. With regard to the relation of angina lacunaris to pharyngeal diphtheria, the author maintains that every case in which there is idiopathic necrosis in the pharynx is true diphtheria, though the presence of false membrane is not of itself a conclusive evidence of such a condition. True diphtheria must be sharply differentiated, however, from angina lacunaris, though neither the micro-organism of diphtheria nor that of angina lacunaris has as yet been discovered. The latter disease is often seen at a time when true diphtheria is not prevalent. In all cases in which there is neither false membrane nor necrosis of the pharynx inspection alone will furnish no sure means for differentiation. Peritonsillar abscess and herpes labialis do not occur in true diphtheria, while paralysis of the velum palati rarely occurs in angina lacunaris. Under all circumstances, in either disease, isolation of the patients should be insisted upon. (*Archives of Pediatrics*, September, 1887.)

**The Treatment of Rachitis with Phosphorus.** Stärker. (*Centr. f. Kinderh.*, 16th April, 1887).—This paper is an analysis of twenty-three cases of rachitis, most of them severe ones, which were treated with phosphorus

in the polyclinic of Prof. Thomas at Freiburg. The formula was the well known one of one centigramme of phosphorus to one hundred grammes of cod-liver oil, to be taken in coffee—spoonful doses, twice daily. During very warm weather the proportion of five milligrammes of phosphorus to fifty grammes of cod-liver oil or olive oil was used. Every other form of treatment, with the exception of that pertaining to the regulating of the diet and hygiene, was intermitted while the phosphorus was used. It was noted that in some of the cases benefit was derived from phosphorus after other means of treatment had failed. The age of the patients varied between a few months and four years. The total quantity of phosphorus used in a single case varied between one and six centigrammes, and the following data will show that individual symptoms were differently affected in different cases. Disturbances in the digestive organs were unaffected in eighteen cases, in four were improved, and in one were made worse. In fifteen cases there was a favourable influence in respect to ossification of the skull, in one case the influence was slight. In fourteen cases the influence upon the development of the teeth was favourable, in five it was not positive. Deformities of the thorax were influenced nine times favourably, and seven times very little, and one time not at all. The shape of the epiphyses was improved in twenty-one cases; in one no improvement was perceptible. Of spasm of the glottis, one case was improved; in one other the result was doubtful. Sweating of the head was improved in five cases; not benefited in one. In eight cases of restlessness and night terrors there was a favourable, in two an unfavourable, result. In eleven cases in which the extremities were crooked there was a favourable result. In eleven cases in which the extremities were crooked there was a favourable result; in four it was not positive, and in two negative. Capability for locomotion was improved in seventeen cases; in two it was not positively demonstrated; in four it was negative. Bronchitis was benefited in four cases; the result was not positive in one; it was negative in five. The general condition was benefited in eighteen cases; not positive in three; negative in two. Those cases in which scrofula was prominent as a complicating element were cured if cod-liver oil and an improved *régime* were simultaneously used with the phosphorus, while in cases in which the former agents were not used the rachitis was cured, but the scrofula persisted. Of the twenty-three cases which were treated, eleven were cured, bronchitis and scrofula continuing in three of them; seven were much benefited, scrofula and spondylitis continuing in two of them; four were slightly benefited; in one the result was negative. Improvement was usually noticeable in two to three weeks after the phosphorus treatment was commenced. (*Archives of Pediatrics*, September, 1887).

**Tuberculosis of the Skin and Mucous Membranes.** Schwimmer (*Rev. Mens. des Mal. de l'Enf.* [abstracted], June, 1887).—The author endeavours to demonstrate that the proposition of Volkmann and Friedländer, that lupus is a local tuberculosis of the skin, is not absolutely true from a clinical point of view, and that the two processes may be clearly distinguished from each other, notwithstanding the fact that the bacilli of tuberculosis may constantly be found in lupus. The two diseases should be distinctly separated for the following reasons:—

- (1.) True tuberculosis of the skin is very rare, while lupus is relatively frequent. Besides, the mode of development in the two processes differs.
- (2.) Contrary to lupus, primary tuberculosis is almost exclusively observed upon the mucous membranes, whence it extends to the cutaneous surface.
- (3.) The lupous and tuberculous dermatoses do not have the same bearing upon the general condition; tuberculosis of the skin is always an indication of an infection of the entire organism, and is always terminated by death, while lupus may persist from childhood to advanced life without causing the development of tuberculosis of the lungs.
- (4.) The bacilli themselves do not bear the same relations in lupus and in tuberculosis. In the former their number is very limited; in tuberculosis of

the skin they are found in large numbers. It is true that there is no morphological difference between the bacillus of lupus and that of tuberculosis; but the same may be said of the bacilli of the two diseases, lepra and syphilis. If the bacillus of lupus were identical with that of tuberculosis, inoculation of tuberculous matter ought to produce lupus. The diseases are, therefore, far from being identical, even from a bacteriological standpoint.—(*Archives of Pediatrics*, September, 1887.)

**Erythema Nodosum Malignum.** Schmitz. (*Centr. f. Kinderh.*, 2nd April, 1887).—In 1872, Uffelmann reported fourteen, and in 1876 the same author reported three more, cases of an ominous disease of childhood, which located itself in the skin, and had cutaneous symptoms which resembled those of erythema nodosum, though it evidently was not that disease. The fact that there were severe disturbances of the general condition which lasted for weeks or months, the occurrence of the disease in children of tuberculous family history and with a decided scrofulous habit, led Uffelmann to conjecture that he had in hand a disease which was akin to both tuberculosis and erythema nodosum, and this opinion was strengthened by the fact that in three of these cases tuberculosis developed within five years. Oehms strengthened Uffelmann's position by a case he reported in which acute miliary tuberculosis with fatal issue developed six weeks after the disappearance of a severe attack of erythema nodosum. The author of this paper is not quite satisfied from Uffelmann's description of the general symptoms in his cases that erythema nodosum was present. If one bears in mind that erythema nodosum is not a local affection, but a general one and probably of an infectious nature, it need not cause surprise if it sometimes runs a very severe course, nor that it is especially liable to affect those who are of a scrofulous taint in whom other infectious processes and the acute exanthemata run a relatively severe course. It was also thought that the subsequent occurrence of tuberculosis had nothing especially significant about it, for a great many of the infectious diseases of childhood prepare the way for the development of tuberculosis at a subsequent period. However well or ill grounded Uffelmann's supposition may be as to the existence of a new disease, the author reports three cases which had the symptoms which Uffelmann described, and two of them resulted in tuberculosis. The symptoms, in brief, were a prodromal stage of three or four days, then an eruption upon certain portions of the skin characteristic of erythema nodosum, irregular rise of temperature from 35·1° to 40° C. during the course of the eruption with severe cephalic and gastric phenomena, and symptoms of catarrhal disorder of the respiratory passages, repeated recurrences of the eruption without apparent cause therefor, and prolonged convalescence. The entire duration of the erythematous disorder was three weeks in two of the cases, and six weeks in the other.—(*Archives of Pediatrics*, September, 1887).

## GYNÆCOLOGY AND OBSTETRICS.

By R. STEVENSON THOMSON, M.B., C.M., B.Sc.

**Bantock on Listerism.**—In a paper read before the British Gynæcological Society, 26th January, 1887, Dr. Bantock ventures to predict the future of Listerism in the following words:—"Finally, gentlemen, what is to be the future of this system? Shall I play the rôle of prophet, and attempt to forecast its future? The old adage forbids. Perhaps, ere many years have gone over our heads, Listerism will already have become a thing of the past and as a tale that is told; perhaps the men of even the next generation, in the course of their study of this subject as matter of 'ancient history,' will be heard asking of each other the question, 'What was it all about?' And perhaps it will be chronicled as one of the crazes to which, to our humiliation be it said, our profession has been given up soul and body. And as we now smile at the

doctrines of the rationalists and the empiricists, of the dogmatists and the methodici, as we are lost in wonder and amazement at the belief in charms and amulets, and in the efficacy of the royal touch, and as we ridicule the vagaries of the alchemists and the astrologers, of the mesmerists and the healers by faith and prayer, as well as the antics of the African medicine man, so will our sons, perhaps, smile at the credulity of their fathers, and wonder at their unreasoning faith in the virtues of a practice which 'was based upon a hypothesis that was not proven,' and certainly was 'not true.'"

**The Treatment of Vaginismus.**—Dr. More Madden read a paper on the above subject before the Obstetrical section of the Academy of Medicine in Ireland, January, 1887. He gave it as his opinion that vaginismus was generally caused by neuromata occurring in the twigs of the superficial perineal branch of the pudic nerve. Many cases could be cured by forcible dilatation of the vaginal canal and stretching the pudic nerve, combined with general sedative treatment. In some cases Sims' or Emmet's operation might be necessary. In conclusion Dr. Madden pointed out that, even in the worst cases, the condition did not necessarily prevent impregnation.

**Oophorectomy in Neurotic Affections.** (Schroeder.)—Removal of ovaries for neurotic affections Hegar divides into two classes:—1st, When the ovaries are plainly diseased; 2nd, When the ovaries are evidently healthy. The indications for the first class of cases are evident; in the second class, however, there are objections to removing the ovaries, and Hegar is of opinion that the operation should only be performed when one or both ovaries are in a pathological condition. Schroeder is of a different opinion. It is not known whether neurotic affections are influenced by the pathological state of the ovaries, or, if they are, how far the connection exists. It is well known that very marked nervous troubles are lessened or done away with at the menopause, either artificial or natural, although there may have been no marked pathological modifications of the ovary. It is difficult also to draw the line between the normal and diseased conditions of the ovary.

Many cases of removal of the uterine appendages for neurotic conditions have been reported, with a success in the majority of cases which would point to the operation being quite justifiable. The main point is to define exactly what are the suitable cases.—(*British Gynaecological Journal*, May, 1887.)

**Is Oophorectomy in any case Curative?**—A discussion on this subject was evoked at a meeting of the Surgical Society of Paris, by the exhibition of a patient whose ovaries had been removed five months previously by Dr. Pozzi for acute abdominal pain which had resisted all the more routine methods of treatment. At the operation the ovaries were found tender, prolapsed, adherent, and full of small cysts, some of which contained blood. Patient recovered rapidly, and when seen by the Society was in excellent health.

M. Terrillon related a case of a similar nature in which the result had been very satisfactory.

Several other gentlemen present related cases occurring in their own practice, in which the removal of the ovaries had been effected with varying results so far as the amelioration of the patients' symptoms was concerned.

In the opinion of M. Séé good results were to be looked for only in such cases as showed coarse pathological lesions in the ovaries.

Those who took part in the discussion seemed to think that relief could frequently be afforded to the patient by the operation when all other means had been tried and failed.

Dr. Gautier described a case of vaginismus at a meeting of the same society in which a cure had followed on the use of electricity. The woman had been married two and a half years, and had had various remedies tried without any benefit. There were the usual signs of vulvar hyperæsthesia, and no lesion

could be found in connection with the anus or vulva. In treating the case the negative pole of a Faradic battery was placed in the vagina, the positive being applied to the sacro-iliac articulation.

After four of these applications the vaginal examination caused no pain.

The treatment lasted one month, at the end of which time the patient seemed quite well, and returned to her husband. Her subsequent health was good, and the cure complete.—(*British Gynaecological Journal*, May, 1887.)

**Cystic Ovaries removed for Dysmenorrhœa.**—Dr. William Duncan showed, at a meeting of the Obstetrical Society of London (8th Oct., 1886), the ovaries of a woman, aged 23, who had suffered from intense dysmenorrhœa ever since the onset of the catamenia. Various methods of treatment had been tried without result. Six months after operation she was in perfect health, and free from all pain.

In the course of the discussion Mr. Alban Doran maintained Dr. Duncan's suggestion that the pain was due to the cystic condition of the ovary, and remarked further, that he believed that through some unknown influence, which might some day be explained, a cystic tumour of the ovary affected the general health of the patient before it produced any pressure effects. A very large, solid, uterine myoma often altered a patient's expression but little, if at all; whilst a woman subject to ovarian cystic disease often presented a characteristic appearance, described by Spencer Wells and others, long before the tumour gave her trouble. Mr. Doran admitted that his statement was hypothetical, but the ovary exerted a considerable influence on the organism when healthy, so that it was reasonable to infer that it also exerted a similar influence when not healthy. He admitted that cysts were found in the ovaries when no symptoms had existed, but there were many varieties of cysts. Where constitutional effects were observed, there was probably change in the stroma, as well as in the ever-growing cyst itself. A tense cyst in tough stroma might reasonably be expected to cause pain in the ovary as elsewhere; and, as had already been suggested, there were probably physiological as well as mechanical effects produced by the growth of a multi-locular cyst.

**Mercurialism in Lying-in Women undergoing Sublimate Irrigation.**—In a paper with this title read before the Obstetrical Society of London, Dr. Dakin gives the symptoms observed in several cases of mercurial poisoning, with the *post-mortem* appearances in one, and a comparison of the results obtained from somewhat different modes of using corrosive sublimate solution as a douche. After giving the results of an investigation as to the general opinion of authorities on the Continent, the author says:—"The probability is that it is exclusively during the first two or three days that the patient is more liable, under ordinary conditions, excluding post-puerperal affections, to both septic and mercurial poisoning. The symptoms may occur later, but this is the period in which the poison, of whatever description, is absorbed.

The difficulty is to hit upon the exact proportion of mercury which will ensure safety from sepsis, and not be too energetic and carry its action beyond this. I think, therefore, that, as far as my experience goes, 1-4000 is quite of sufficient strength for the first four days, used twice daily. After this the strength may be diminished still farther, or the douche used only once daily. Immediately before and after labour the 1-2000 may still be retained, all the precautions being strictly observed, and scrupulous disinfection of the hands of all concerned being practised. In all cases the constant and unvarying stream obtained from a douche tin raised about three feet above the patient should be used in preference to the more or less intermittent flow from a syringe.

In cases already infected, and for curative purposes, stronger solutions, 1-2000 or even 1-1000, may be admitted, either as vaginal or intra-uterine, but, of course, should always be administered with the greatest caution and by the physician himself.

*Books, Pamphlets, &c., Received.*

- An Index of Surgery. By C. B. Keetley, F.R.C.S. Fourth Edition. London: Smith, Elder & Co. 1887.
- On the Animal Alkaloids, the Ptomaines, Leucomaines, &c. By Sir William Aitken, M.D., F.R.S. London: H. K. Lewis. 1887.
- Anæmia. By Frederick P. Henry, M.D. Philadelphia: P. Blakiston, Son & Co. 1887.
- Surgical Pathology and Morbid Anatomy. By Anthony A. Bowlby, F.R.C.S. With 135 Illustrations. London: J. & A. Churchill. 1887.
- The Diagnosis and Treatment of Eczema. By Tom Robinson, M.D. London: J. & A. Churchill. 1887.
- Pathology and Treatment of Ringworm. By George Thin, M.D. London: J. & A. Churchill. 1887.
- Dental Caries and its Prevention. By Henry Sewill, M.R.C.S., and L.D.S. Second Edition. London: Baillière, Tindall & Cox. 1888.
- Manual for the Physiological Laboratory. By Vincent Dormer Harris, M.D., and D'Arcy Power, M.A., M.B. Fourth Edition. London: Baillière, Tindall & Cox. 1888.
- Clinical Lectures on Mental Disease. By T. S. Clouston, M.D. Second Edition. London: J. & A. Churchill. 1887.
- Practical Lessons in Nursing:—Maternity, Infancy, Childhood. By John M. Keating, M.D., Philadelphia. Edinburgh: Young J. Pentland. 1887.
- Practical Lessons in Nursing:—The Nursing and Care of the Nervous and Insane. By Chas. K. Mills, M.D., Philadelphia. Edinburgh: Young J. Pentland. 1887.
- Practical Lessons in Nursing:—Outlines for the Management of Diet. By Edward Tunis Bruen, Philadelphia. Edinburgh: Young J. Pentland. 1887.
- A System of Gynecology. By American Authors. Edited by Matthew D. Mann, A.M., M.D., Buffalo, N.Y. Vol. I. Illustrated with 3 Coloured Plates and 201 Engravings on Wood. Edinburgh: Young J. Pentland. 1887.
- A Manual of Treatment by Massage, and Methodical Muscle Exercise. By Joseph Schreiber, M.D. Translated by Walter Mendelson, M.D., New York. Edinburgh: Young J. Pentland. 1887.
- The Life and Recollections of Doctor Duguid of Kilwinning. Written by himself, and now first printed. Edited by John Service, L.R.C.S. and P. Ed. Edinburgh: Young J. Pentland. 1887.
- A Compend of Human Anatomy, including the Anatomy of the Viscera. By Samuel O. L. Potter, M.A., M.D., San Francisco. Fourth Edition. With 117 Illustrations. Edinburgh: Young J. Pentland. 1887.

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ORIGINAL ARTICLES.

LECTURES ON THE DISEASES CLASSIFIED AS  
TABES MESENTERICA.

By W. T. GAIRDNER, M.D., LL.D.,  
Professor of Medicine in the University of Glasgow.

(Concluded from page 358.)

CASES IN ILLUSTRATION, WITH REMARKS—*Concluded.*

CASE IV.—(*Medical Times and Gazette*, 17th October, 1885, p. 525.\*)—*Very well-marked thickening, amounting to tumour of the great omentum, gradually resolved in two years, with extremely insignificant symptoms. Crepitus in right pulmonary upper lobe, arising under observation, and persisting during the entire course of treatment with little modification—Symptoms scarcely appreciable, and general health well maintained.*

Lizzie (or Elizabeth) C. was 9 years of age when she was first brought under my notice on the 7th of May, 1883, on account of a swollen abdomen; which, it may be

\* This case, from its great importance, was recorded in the *Medical Times and Gazette* with the most anxious care to convey, if possible, the exact verbal impressions, corresponding with a great number of separate clinical demonstrations and lectures to successive classes of medical students, during the two years she was more or less under observation up to October, 1885. It is, therefore, considered expedient to preserve here the very words of the original publication, without any attempt at condensation, and omitting only an introductory paragraph.



said quite unreservedly, was at that time her only symptom, and, even after most careful examination and enquiry into the facts, was almost the only thing that could be ascertained about her suggestive of organic disease at all. Now, a swollen abdomen in a child may proceed from many causes, and is often regarded as a very trivial and unimportant disorder, *per se*. I do not say that you will be right in so regarding it; I would rather lead you to be very careful about all such cases, and not to dismiss them in the summary way they are often treated, with a little grey powder or a few doses of castor oil. No case could have been more trivial than that of Lizzie C., if judged by the amount of local uneasiness or of constitutional disturbance; for, from a note extending to more than seven pages in the Hospital Journal (Ward 6, T., pp. 222, *et seq.*), and mostly made by myself in presence of the class within two days of her first admission to the Western Infirmary, we can assure ourselves even now that the most thorough investigation was made at that date, and that neither in respect of complaints, nor in respect of physical and physiognomic signs, was there anything to beget even a suspicion of grave disease, except the physical condition of the abdomen; no hereditary syphilis, no scrofulous indications, no rickets, no chest symptoms of any importance, unless it be the cicatrices of a very old and obviously artificial eruption which may have pointed to some chest disease in early infancy, treated with antimonial or other ointment, but in the interval of years practically lost sight of and forgotten. She was said to have been always a healthy child, and her appearance confirmed it; she was, indeed, the very picture of health, nay, even of superfluous or "rude" health (as judged by her colour and general appearance) at the time of admission. She herself had first drawn attention to the swelling and hardness in the abdomen about three months before, and she admitted that she had felt it "slightly sore." Her mother affirmed that, a few days after the swelling was first noticed, the girl was troubled with diarrhoea, at first pretty severe; but this had ceased after a fortnight, under merely dietetic treatment and without medical advice, and it had not recurred. Appetite and digestion, according to her mother's testimony as well as her own, remained unimpaired; and I may add that our experience in the ward tended in all respects to confirm these statements, in respect that her temperatures, carefully taken for weeks together, showed

no signs of fever; and her body-weight, by no means apparently deficient even at first, went on increasing after admission.

And yet, with all these facts in her favour, more strongly so even than in the case of Jane M. (the first of this series that I placed before you), there was no doubt at all, in Lizzie C.'s case at this time, of the existence of a distinct tumour in the umbilical region, corresponding exactly, both as regards percussion and palpation, with the characters of a thickened great omentum as already detailed to you. The report, which goes into every element of the diagnosis as demonstrated to the class on 9th May, is open to your inspection; and I spare you the details here, simply because there is so much besides in the case to occupy us. The tumour was distinct and palpable, dense to the feel and superficially dull over the entire umbilical region, so that it could not have been either fæcal accumulation, or merely inflated intestines. The epigastric, hypogastric, and both iliac regions were comparatively soft and yielding, and quite normal to percussion. There was not, in fact, a single element in the diagnosis of thickening of the great omentum that was wanting in this case, and all the physical signs were present in even a more definite form in Lizzie C., than in any of the others; but there was no evidence at all of fluid effusion at the time we first saw this girl, nor at any time afterwards. We judged it possible or probable that there might have been such effusion at an earlier date; but if so (as in the case of Jane M.), it had been absorbed so as to leave no traces to physical diagnosis.

Had the case ended favourably at this point, and without any observations of interest other than those above mentioned, the details would have been mainly a reproduction, or rather an anticipation, of the facts presented to you in Jane M., with some quite non-essential variations. Had this been so, I should probably have been satisfied with alluding to it briefly as such. But there is much more of clinical instruction in the case than this. The great probability, from the pathological point of view, of such a lesion as existed in Lizzie C. being of tubercular origin led to a more than usually, indeed an anxiously careful exploration of the lungs at the very first examination (May 9th). The result of this was that there was some ambiguity as regards the auscultation and percussion signs, respectively, of the two apices; but on the whole we thought the right apex showed "*a distinct deficiency, both*

as to quantity and quality, of the R. M., and even, perhaps, a little irregularity amounting to jerking. At the back," the report continues, "percussion of the right apex appears relatively impaired, and here also the R. M. is *inferior in fulness to that of the left, but without râle or other alteration in quality.*" I emphasise one or two points in this report for a reason that will presently appear. Taken by itself, it amounts to a suspicion, but no more, of disease, probably tubercular, in the right apex.

Just a week after this, however (16th May), I was again examining this patient, after noting carefully her satisfactory condition since admission as to the general symptoms. On this occasion my attention was attracted for the first time by "a fine and slight crepitus, probably of the mucous order, developed in the right apex since the previous careful observation, and audible very distinctly down to the second, and less so down to the third rib, accompanied everywhere by wheezing râles, but of no great intensity." The observations, in other respects, as to the relative fulness of the R. M., &c., on the two sides, were confirmed on this occasion.

I purposely caused a large number of persons to listen to these sounds, because, although it was possible that they might have been merely occasional and transitory, and therefore of no great importance, it was also possible that they might have a bearing on the diagnosis and prognosis of the case as one of tubercular disease both in the abdomen and the chest. The observation being made thus early and thus studiously with reference to this point, was followed up with the more freedom, because the state of the patient admitted easily of frequent examination. She was gaining in weight; she was free from fever and from pain; she had no considerable cough,\* and no expectoration. Yet from that time onwards I can absolutely affirm that during the whole summer, when she was under daily observation up to the end of September, 1883, there was not a single occasion when the râle above referred to was found absent from the right lung in front, or even very difficult of detection. It had always nearly the same characters—a very fine clicking or crepitant râle, always much more

\* The precise fact, as noted, 16th May, is as follows:—"The ward sister, on being appealed to, has noticed a short cough without expectoration, but not such as to molest her, or to have attracted the child's own attention, which, therefore, may have been indefinitely present before admission."

distinct during inspiration, and sometimes only audible with forced inspiration, without any appreciable amount of tubular or other altered quality of the R. M. in the right lung; but with, as above stated, a somewhat depreciated relative amount of fulness, and this both at apex and base. It perhaps differed from the crepitus of the early stage of pneumonia, but very slightly so, in being more of a moist character; and certainly much more in its long persistence without other alterations. All this was frequently the subject of clinical commentary, and of very repeated observation, not only by myself and my assistants but by some of the other physicians of the Western Infirmary, and by occasional strangers. In not one instance did the râle altogether fail to be observed, although its area and its amount both varied. It was chiefly heard over the upper lobe, and never on the left side.

During her residence, on this occasion, of nearly four months, Lizzie C. gained in weight, in all,  $25\frac{1}{2}$  lbs., having been on admission 3 st. 2 lbs., and at her dismissal, 4 st.  $13\frac{1}{2}$  lbs. The condition of the abdomen very notably improved, without its being able to be said that the physical signs mentioned had altogether disappeared. Her cheerfulness, and her sense of well-being, were unimpaired throughout, and no local symptom seemed ever to cause her any uneasiness. Her cheeks were almost preternaturally rosy and fat, and it was only by becoming satisfied of the entire absence of fever that we acquitted her at times of a morbid flush; but I afterwards came to know that this peculiarity, and her altogether exceptionally good physique, had procured her an engagement at a theatre as a subordinate in a pantomime, and were by her mother and all her friends considered to be exactly in accordance with her condition from infancy onwards. I may add, that although partial sweats about the head and face appear once to be mentioned in the report, it was during warm weather, and was not considered, ultimately, to have been a fact of any importance.

Here, then, we have a combination of details, in the case of Lizzie C. which I have given you, so far, in a much condensed form, or abstract, from journals T and U of the female ward, inviting you to refer to these journals for yourselves, in case you should be of opinion that all the evidence of importance is not fully before you. I will merely say here that there is not a single point, in my opinion, in which the evidence of detail bearing on the

peritonæal disease was not as carefully recorded, and as faithfully considered in reference to possible fallacies, as in any of the other cases here cited to you, or, as in the case of Jane M. and Mary Jane S., which have been mainly under your own observation. And you are not to suppose for a moment that any of these details are at all new to me, as I may presume some of them are to you.\* On the contrary, cases of this kind in a general way (though that of Lizzie C. is certainly an exceptional one) have been constantly occurring to me for more than thirty years in hospital and dispensary practice, as well as in consultations among the more affluent classes; and the impressions about them that I wish to leave on your minds might be almost indefinitely illustrated out of the fifty or sixty journals which a decennium of the Western Infirmary has accumulated for our wards, as well as by reference to older experience in the Royal Infirmary here, and in the Edinburgh Royal Infirmary. What I wish to do now, however, is not to generalise prematurely, but rather to place before you the lessons of these four cases, thus successively presented in series, as elements out of which you may learn something more for yourselves. And it is to press home the lessons of Lizzie C.'s case that I now invite you to read with me more continuously the later reports, made when she could be hardly spoken of as a patient, but rather when she was sent for at my request, in order to take stock (as it were) of her physical condition and progress.

On 1st February, 1884, I sent for this girl (she lives at Paisley, seven miles from this), and in the journal of Ward 6 (V, p. 169) we have a note which, as it is not long, I will place before you entire:—"Speaking generally, it may be said that all the facts noted last summer are more or less apparent, the abdomen being soft and elastic, but decidedly abnormal to percussion over the whole omental region; and the true intestinal note (*i. e.*, on superficial percussion) being only procurable towards the hypogastrium. The râle observed in the right apex is still appreciable, and perhaps more clicking and hollow in character, but still not so

\* Although in teaching clinically, I usually make but little reference to authorities, in dealing with demonstrable facts, it would be easy to show that in most of the text books the diagnostic characters and significance of thickening of the great omentum have been strangely overlooked; although the mere anatomical fact has long been known. Hence the prominence given to these points in the preceding papers.

definitely altered as to suggest any well marked new physical changes. The mother's account is as follows:—Lizzie complains of nothing, is always light hearted and fit for all games with her companions, rising at eight in the morning and going to bed at eight in the evening without any appearance of undue languor, sleeps well at night, takes her food well, is never sick, has no diarrhoea and complains of no pain. As regards pulmonary symptoms, her mother remarks that she may take a cold occasionally, and did so three weeks ago; and as the result of this a very little cough may be admitted as occasionally audible, but not so as to make the girl herself complain. [See footnote above, indicating exactly the same facts as at date 16th May, 1883.] She has preserved to the full the rosy appearance she had in summer—so much so, that the only question that can be raised is whether it does not incline to lividity. Her mother thinks her decidedly less fat than when dismissed last summer, but she has never noticed any feverishness nor sweating.”

Again, on the 11th of October, 1884, I sent for Lizzie C., and on this occasion, and some later ones, much longer and more elaborate reports will be found in Journal X, p. 37, *et seq.*, the substance of which (condensing them very carefully to save repetitions, but omitting nothing essential) I will now endeavour to convey to you.

It appears that Lizzie C. suffered somewhat from headaches during the autumn of 1884; and as they disturbed her sleep she was on this account withdrawn from school in September, and put under medical treatment. There was, however, no uneasiness as regards the chest or abdomen till the beginning of October, when she complained of pain in her right side, and on strict enquiry it appears she was possibly a little short in the breathing, but it was by no means a prominent symptom, and in the main, her mother adheres to the report made on 1st February (see above). She may have been a little feverish with the headaches, but otherwise there was no evidence of any constitutional disturbance. The physical facts were exactly as before, with the exception (which, however, was in exact accordance with the observation of 16th May, 1883) of a certain amount of distinctly wheezing râles audible on the right side. (It was presumed that these were on this occasion of recent origin.)

On the 11th October, 1884, Lizzie C. was found to weigh 5 st. 2½ lbs., as against 3 st. 2 lbs. at our first observation

of her in May, 1883, and 4 st. 13½ lbs., at her dismissal in September; so that although she had not lost weight (but in fact gained about 3 lbs. between September, 1883, and October, 1884) it might be fairly maintained that there had been either an absolute or a relative loss of weight during some portion of the latter period, corresponding, in all probability, with the illness above mentioned. It was, however, by no means very apparent to even a professional eye that she was notably thinner or less in good health than on previous visits.

On 5th January, 1885, this girl was readmitted at her mother's request, chiefly on account of a "shortness of breath," with wheezing, especially at night. At this time she had gained 3 lbs. in weight since October, and looked certainly to my eye as well as ever (as a somewhat critical note made the next day informs me); the trace of "lividity" above incidentally alluded to being discounted (as it were) in consideration of previous knowledge of her appearance. The pulse was 80, regular, and of fair strength. The respiration barely 16 in the minute, and absolutely tranquil. Apart from the mother's statement, there was nothing to lead to any apprehension of renewed chest disease. It was, however, ascertained more fully than previously that Lizzie's father died of something that was called "bronchitis with inflammation of the windpipe" at the age of 30. He was a very intemperate man. Her mother also thought that there were traces, at least, of bronchial affections in the family.

The abdomen was found to have a maximum circumference at this time of 24½ inches, *i. e.*, one-and-a-half inches less than at the time of first admission, and almost four inches less than the absolute maximum noted on 28th August, 1883. Allowing for the developmental growth and improved condition of the child generally, this was considered a highly satisfactory note, and it was borne out in detail by a most critical estimate of the physical facts, which, as it was practically superseded at a later date by a much more compendious statement with almost identical results, I will not trouble you to read over. The rôle, so often previously observed and discussed, was at this date made the object of a formal lesson at the bedside to the clinical class, and in the very ample report framed for the purpose of detailed instruction, and resting on all the previous observations, the following statements emerge:—"Dr. G. remarks, with a view to the classification of this

râle (1) That during the whole period of its singular permanence it has been heard, perhaps not quite exclusively, but always in an immensely preponderating degree, with the inspiration; (2) That while it has at different times and in different degrees suggested so much of the moist quality as to make it impossible to demarcate the râle from the moist râles, there has been no such consistent progress from dry to moist as is implied in Dr. Walshe's description of the dry and moist crackling rhonchi respectively (see 4th edition) *Diseases of the Lungs, &c.* pp. 326-330; therefore, it was argued, there had been no evolution of the phenomena in the direction proper to, and according to Dr. W., eminently characteristic of, the progress of tubercle in the lung towards softening; (3) It appears, both to Dr. G. and Dr. Middleton, that there has been a little (but still only a little) increase in the tubular or bronchial quality of the R. M. in the right apex since the first observation; and this mostly, if not exclusively, in the region of the sterno-clavicular articulation; (4) It is difficult to say whether there has been any change in the percussion since the first observation in May, 1883."

On 4th February of this year (1885) we were able to record the results of a renewed daily observation of this case for a month in hospital as showing once more "quite satisfactory progress" in respect of the removal of the slight complaints indicated on her re-admission. "She says she has now no headaches. The pain on the right side is gone, and she appears to be quite unconscious of any wheezing or other notable symptom of pulmonary disease. Appearance perfectly good; temperatures subnormal rather than otherwise, but cannot be said to show anything worthy of remark. Apart from the history, there would be no reason at all for anxiety about the patient, or for detaining her in hospital. The abnormal facts in the abdomen are still just perceptible, but with so great an amount of difficulty that in the absence of previous history they would certainly be disregarded. It seems safe to say that the palpation is that of a normal abdomen in a plump, well nourished child, with a good deal of fat in the abdominal wall. [The percussion still, however, appreciably abnormal.] The circumference is now  $26\frac{1}{2}$  inches, being two inches of increase since she was last admitted on 6th January, but this increase is probably commensurate with an increase in the general bulk of the body; at all events, there is nothing whatever to lead one to suppose it abnormal.



[The circumference in February, 1885, was in fact just over what was recorded as that of an abnormal or swollen abdomen in May, 1883, the difference being that normal development had taken the place of disease.]

As regards the chest, it was observed on 4th February, 1885, that the same ambiguity which had been noted in May, 1883, existed in the percussion of the two apices. "The well known râle was again found in the right apex in even a larger and more diffused form than ever before, and, though it is difficult to put into words changes of so extremely slight a character, the râle is certainly more of a moist order, and with larger bubbles than on most previous observations, and this is the more remarkable, as it is only a few days since Dr. G. and Dr. Beveridge, examining together, were struck with the paucity of the râle, and with its being rather difficult to catch, except at some points and on very full inspiration. To-day, the râle is distinct at all points down to the third rib, and even occasionally a little below this, but during the examination by the class it becomes much less abundant and more circumscribed, and is chiefly audible in the sterno-clavicular region. Wheezing is scarcely if at all appreciable. [The other characters of the R. M. did not differ from previous examinations; indeed, could not be said to indicate any advance in the disease even as compared with May, 1883.] Sputum has been entirely absent since admission."

On 7th February this girl was dismissed for the second time, after exactly five weeks' residence, greatly improved and practically well. The whole period of observation and treatment, taken together, in this case may be said to extend over more than two years, and it is likely, I hope, to be much further extended, for I have good hopes now Lizzie C. may attain to womanhood, or even to mature age; and, having watched her case with so much interest thus far, I shall endeavour, if possible, not to lose sight of her entirely in the future.

[On a recent occasion, being in Paisley, I had an opportunity of examining Lizzie C.; and although there was not, perhaps, any essential change in the facts, it was evident to me that she leads a life of considerable privation and perhaps of some hardship. I shall, therefore, in all probability, cause her to be admitted again to the Western Infirmary, if so disposed, in the course of the winter. The râle in the right lung, however, was less pronounced than it has frequently been in the course of this report.]

## OBSERVATIONS ON THE STUDY OF DISEASE.

By J. WALLACE ANDERSON, M.D.,  
Physician to the Royal Infirmary.

*(Being the Retiring Address as Honorary President of the Royal Infirmary Medical Society, Session 1886-87.)*

GENTLEMEN,—It has often occurred to me, lecturing in this very room to you on the Practice of Medicine, as controversial or obscure points presented themselves to our notice, that here was an interesting question for our Medical Society, or here again was a good subject for study, or field for argument and debate. Not that I would have you be mere wranglers, diligently discussing the different sides of a question, while the truth, unheeded, makes good its escape. I do not mean that. But we all know that discussion, properly conducted, means thorough searching, complete separation of part from part, that the whole be comprehended as well as it may. And I know that you can and will do this. But discussion, even for its own sake, must be a feature of such a Society as ours. You must take sides, Gentlemen. You must have it out with each other, for you cannot have it out with your teachers; at least not till you have shaken the dust of these class-rooms and wards from off your feet, and then you will generously forget the past.

Last winter, Gentlemen, I heard that there were divisions among you. It was reported to me ominously, but I never knew exactly on what questions you divided. I may presume, therefore, it was on some such points as these:—Is pneumonia a constitutional or a local affection? Is it not a different thing in the hospital to-day from the description you get in the class, drawn rightly enough in part from a past, and possibly a more robust age, than ours? May it not be a name for a great many different maladies? Is chronic rheumatoid arthritis after all a distinct disease from chronic rheumatism? Is such a commonplace affection as lumbago of rheumatic character? What about the identity of croup and diphtheria? These are the kind of questions that, I suppose, you divided upon. At least, it is such questions as these that seem to me becoming to this Society, knowing as I do the energy, enterprise, and ability that are within its ranks.

But it is not on any of these special questions that I mean to address you to-night. I leave that for the class-room. I think it is my duty, as your Honorary President, rather to

suggest than to determine any particular line of inquiry. I would rather present to you, for your own further reflection, some views of disease that have been floating for a time loosely in my mind, but which I have now brought together into some kind of shape; some thoughts whose elucidation or controversion might be considered by you, amongst other things, suitable and acceptable work for the Society during the coming session.

The first remark I would make is of an introductory character. It is this. In all your study of disease endeavour to advance from mere details to general principles. "Make your acquisition of facts subordinate to the attainment of general principles." \* It is twenty years since I heard that from the lips of an old and respected teacher, who was long a manager of this hospital, the late Professor Allen Thomson. I cannot remember if I even caught his meaning at the time, but I have often thought of his words since. Not that there is anything novel or original in the remark, but it is sound advice; and there is such a show of details nowadays in our profession—I was going to say a shoal of details—there is such a show of details, chiefly in the way of case reporting, that we need to remind ourselves that there is something more than that to be done. Of course, we must have details first, but we are not to rest content there. When we have tried our materials, then it is time to build. The whole is greater than its part, not alone because it is greater but because it is complete.

But I must illustrate my meaning. I suppose there is hardly a disease, or at least a particular symptom of a disease, of which some one or other has not gravely remarked, that it is worse at night. Scarcely a week passes but one hears the observation; usually it is one particular symptom, pain, that is meant. Once impressed with the weakness of the statement, I have observed it quoted of, I cannot say how many diseases. The last instance occurred not long ago, when I was turning over the first leaves of a new surgical dictionary. Here I saw that a particular form of *caries* was carefully described as being worse at night, as if that were specially characteristic of the morbid process under consideration; and I believe in accordance with the spirit of the age, our medical age, this fact to which I have just referred has got its own special pathological hypothesis, by which its occurrence is explained in a highly satisfactory manner. And so a specific form of

\* Introductory Address at the Public Opening of the Medical Session, 1867-68, in the University of Glasgow, by Allen Thomson, M.D., F.R.S.

rheumatism is commonly said to be more severe at night, as if that were characteristic of it alone. I am convinced it is not so. It would be much more significant, Gentlemen, much more interesting, if some one could point out to us a disease that is worse during the day. I except of course periodic diseases, which may be most severe at any time in the twenty-four hours. And we need spend no time over the explanation. It is apparent enough that it depends, *inter alia*, on the general law that the vital energy is lowest between midnight and morning.

But now, gentlemen, having your minds directed to that general law or principle, namely, that the symptoms of disease are apt to be most pronounced at night, it becomes an interesting and profitable study for you to search for its illustrations in detail. I shall give you one. It is a feature of such widely different diseases as idiopathic asthma and gout, that their early attacks, or still more a first attack, come on about the same hour, indeed with wonderful uniformity about the second or third hour of the morning. So does spasmodic laryngitis in children, or perhaps a little earlier, nearer midnight. It is needless to multiply examples. There are many that I could easily quote which are less common, but it is common things and common laws that I am speaking about. Obviously the main factor in the case is, as I have said, that from midnight onwards for a few hours the system is at its lowest ebb, and therefore most susceptible to morbid impressions.

Another illustration of a general law is the uniformity of our body temperature in health, and the uniformity in our surroundings which it requires. Or we might formulate it thus:—*Uniformity of body temperature a feature of health; variations in the external temperature a cause of disease.* You observe I purposely take illustrations, that if you have not thought them out and expressed them in so many words, are yet so apparent that they are at once accepted by you. For you all know how wide is the range of temperature in the natural world, and how still greater extremes can be produced and measured artificially. Yet within what a narrow limit is the temperature of the healthy human body contained. Life is measured by a brief enough span as regards the thermometer; not much more than one degree. That is to say healthy life. We almost feel as if the thermometer had reduced the condition of health to a mathematical point. And the range in disease is not great between that of a death-like collapse and of a fever that, if continuous, will soon be fatal.

So, on the other hand, are variations of the surrounding atmosphere a cause of disease. We can withstand great extremes of heat and cold, one or other, if it is continuous and uniform. We cease to a great extent to feel that it is an extreme. We become familiar with it. But vary the one with the other, change them suddenly, and we suffer. "Heats and colds," as our hospital reports have it, they are the fruitful sources of disease. Here again we see but a part of a still wider law, namely, that habit is an essential factor in our whole economy. That we are creatures of habit is a more thorough truism than is commonly supposed.

There is a law which is worthy of our notice, one of limited action, however, and hardly entitled to be called a general law, and that is, *the power of resistance in the human body to a lesser evil when a greater assails*. I think my attention was first directed to this when, as a student of medicine, the picture of the asthmatic sitting at an open window, with his chest exposed to the cold winter air, perhaps for the greater part of a night, was sketched for us by our lecturer. The asthmatic does not take inflammation of his lungs from such a procedure; he does not even take a common cold. So, again, the amount of exposure to direct cold, and even to wet, that is safely borne in a case of severe hæmorrhage, where these agents are employed for its arrest, is very striking. I cannot bring to my recollection many very convincing examples of this law; but the immunity with which fever patients, as we see particularly in Continental practice, may be immersed in cold water, is probably explained in the same way. There, it must be admitted, the explanation seems to be more obvious. The fever and the cold are simply antagonistic. But in cases like the two first mentioned the explanation, I think, lies too deep for us. Sir Thomas Watson accounts for these and similar phenomena by saying that "impressions which are unheeded are unfelt and inoperative;" but this, I think quite clearly, will only apply to the conditions of our animal life—*i. e.*, the actions and sensations in which our intellect is or can be concerned. It will not hold true with regard to what I can only call our organic life. A man will catch a fever quite as readily though he is unconscious of being exposed to the infecting particles, or get a lumbago though he was unaware of the draught at his back.

Leaving, now, illustrations of general laws or principles, I shall pass on to the consideration of some features of disease that you may think suitable for further study. The first I will put in the form of an antithesis, thus:—*The weak point*

*in the organism; the special point of attack in the disease.* The more I reflect on our relation to disease, the more am I struck with its resemblance to a contest. Some of you have heard my opinion on the subject already, but not on this aspect of it. In all of us—however strong our bodies, however vigorous our constitutions as a whole—there is the weakest point. It is not in nature to construct an organism of equal resisting power throughout, for nature is never perfect in detail. And we are, after all, only as strong as our weakest point; or, as the proverb has it, "The chain is only as strong as its weakest link." You must have asked yourself the question, Why does one man take a pneumonia, another a pleurisy, and another acute rheumatism, each brought about, we shall suppose, by the same cause—a thorough wetting? The thorough wetting has struck at the weakest point. The flaw, the taint, the idiosyncrasy is somewhere, and that is the weakest point. So much for the defence.

Against us we have not only special diseases, each with its own particular mode of action, but general diseases with their own special points of attack—the throat in diphtheria, the bowel in enteric fever, the kidney in scarlet fever, and so on. And special tissues too: in one disease the fibrous, in another the muscular, in a third the nervous textures. With such flaws in the armour, and exposed to attack on every side, the contest with disease is carried on.

I would now, Gentlemen, direct your attention to a point worthy of consideration in the investigation of disease, which we may best formulate in a way that possibly at first looks more like a catch than anything of real moment. It is this. *Different cases of the same disease may present very different symptoms; and very different diseases, necessarily in different cases, may present the same symptoms.*

This proposition, whose truth a moment's reflection renders very obvious, is often forced upon our attention in the endeavour to identify or diagnose strictly a particular disease. We see the same disease taking, as it were, many an *alias* in the attempt to avoid detection, and a very different, and perhaps a very dangerous disease concealing itself under the cloak of one which, for the sake of the illustration, we shall suppose to be a comparatively trifling offender. Perhaps you will understand this better if I once more illustrate what I mean.

Take such a complaint as epilepsy. How greatly it varies in character and in degree. The attack may be severe and prolonged, or it may be so slight as to amount to no more

than a momentary break in the line of thought. In its minor forms certainly, it appears under such a host of *aliases* that authorities have sought in vain for a satisfactory test of its genuineness. Loss of consciousness, as laid down by Reynolds and others, is probably as good as any, but we know that such a criterion will by no means include all its forms. As merely a momentary confusion of thought, or a slight tetanic spasm of a set of muscles, it may altogether defy detection till we get a hold of its associates; till we find that it comes out of a bad nest, to wit, insanity, or other forms of nervous disorder.

So with regard to enteric fever. What a variety of types it presents to our observation, and how utterly hidden it sometimes lies under a cloak of vague and conflicting symptoms. You look at the case and you know that it must be enteric fever or not that at all. If not that, it cannot turn into it, it cannot be half it, it must be something quite different, and yet you know it *may* be genuine enteric. You must be aware by this time that such forms are common in children, and might be absolutely beyond reasonable proof of identity, were it not that the fever is epidemic in your district; the undoubted cases in the adult proving the interpreter to the obscure forms in the child. Some, indeed, still believe in, and older writers have described, a distinct gastric fever; a fever which is not a mild enteric, but a different malady altogether. But I need not multiply examples. I feel sure that in your study of disease, you will find ample illustration of the first part of our proposition, that different cases of the same disease present many different symptoms, even to the extent of defying diagnosis.

But the proposition loses whatever force it has without its second part, which we have yet to notice, namely, that very different diseases may in different cases present the same symptoms. Scurvy and purpura are a striking example of this. No two diseases could be more distinct than these: one clearly dependent on a certain dietic privation, the other in many cases, if not in all, defying any explanation. And yet they may present almost identical symptoms. The history of medicine furnishes us with many examples of different diseases presenting similar symptoms, and being for ages considered identical. Gout and rheumatism were thought to be the same disease till Sydenham proved the contrary: and strange as it seems to us now, measles, small-pox, and scarlet fever, must have been at one time very imperfectly, if at all separated. So, as you are aware, have typhus and enteric fever been quite recently distinguished. All this has been

effected by remembering, consciously or unconsciously, this second part of our proposition.

I have long believed that pneumonia is but a term for several diseases of like symptoms. We have the old classical form, essentially non-catarrhal, sometimes epidemic, sometimes, it has been thought, infectious. Without being too speculative, we may suppose it possible, or even probable that two distinct diseases have been included under the one name, one infectious, the other not. In more recent times the catarrhal form has been well recognised, while unquestionably there is a traumatic inflammation of the lungs, so closely resembling the idiopathic form that its distinctive features afford us still an interesting field for further study.\* In fine, I think pneumonia is a term that includes several different diseases, which have in common, certain well defined symptoms and signs.

Whether you consider this comes within the scope of practical medicine or not, Gentlemen, I venture to think that while you employ an odd leisure hour in puzzling yourselves about the nature of some obscure medical disorders, the proposition to which I have been directing your attention will recur with some advantage to your memory. For it will not do to say that a disease is recognised by its symptoms alone. They may be equivocal, but the disease cannot be. And while, after all, the best may be baffled, he who can take the widest view of all the past history, and of the whole present conditions bearing upon the disease, will be the one who will arrive nearest to the truth.

I have said all the past history and the whole present conditions. Do we sufficiently consider what these words mean? I am going to bring my observations to a close by indicating what they mean.

You will often find yourselves sorely exercised over the exceptional course or the unexpected turn that an apparently simple disease takes. You are called to see a man who, though he is only forty-five years of age, is evidently past his best. There is some pulmonary mischief. He has not a very high temperature, little pain, and a non-characteristic spit. There is little or nothing in the account he gives of himself to explain why he is ill at all. On examination you find slight dulness of the chest wall over by no means a wide area and unilateral, and a not very characteristic pulmonary *râle*. But he dies when many a patient with more pronounced acute

\* I have considered this question from another standpoint in a paper on "The Specific Origin of General Disease."—*Lancet*, 2nd May, 1885.



disease recovers. Or, again, your patient, who has been getting gradually worse, has at length extreme dyspnoea, præcordial oppression, general dropsy, cyanosis, and by and bye delirium. You know that both heart and lungs are grievously affected; but he does not die, at least not yet. It is impossible he can recover; but he rallies again and again before he falls in the unequal contest.

Why do these cases run a course so unlooked for? Not so different a course; that is simple enough. But why did the first die when we think he should have recovered, and the second return again and again to the encounter, as if he mocked at death? Where is our science, where the resources of our art, that we cannot explain this? Such thoughts, I doubt not, will often cross your minds. Gentlemen, if we knew all the past history of each case, it would be simple enough. We may not need to go back very far. In the first case, it may have been a constitution weakened by excess. No; he was a temperate man. You try again, and you find his work was not of a specially injurious kind. At last you discover that the excess was overwork. Or it may have been no work, and the natural consequence, privation. The man has been starved.

But after we have exhausted our inquiries and failed, are we to reproach ourselves? Have we, can we have, the whole past history? Can he tell us every event of his life; and if he could, could we appreciate the significance of each?

And the other man. Why did he not die when you thought he would; when all your experience led you to think he would? Why was he able to contest inch by inch and even gain an advantage, though, alas, only temporary, against such overwhelming odds? Here, again, we would need his whole history. We enquire into every detail, and find his life was well ordered. But, perhaps, even it was not all it might have been. We go farther back and we find that his parents bequeathed to him the best of all inheritances, a faultless constitution. The malady had been repulsed by the inherent vigour of the constitution.

I hope, Gentlemen, I am giving a sufficiently practical conclusion to my remarks by impressing these things upon you. You will find many examples of such puzzling, contradictory cases, as it were. Remember there must be an explanation somewhere, and I have been showing you one way along which you should seek for it. I have long thought, and some of you have often heard me teach it, that the most practical question, a vital question, for us in a case

of pulmonary consumption, is—Has it been induced or is it inherent? Has it been induced by adverse extrinsic conditions, or is it from an inherent depravity of constitution? In nine cases out of ten that will be the main element in the prognosis, and in the exceptional instance will only be second in importance to the particular stage at which the disease has arrived. In hospital practice you more usually find that the mischief has been forced upon our patients by their unhappy lot. Give them rest and food. Give them a chance of life, or let them give themselves a chance where the fault has been greatly their own, and they will do well. They will recover, when the rich, on whom the disease has fallen in spite of their wealth, will not be rescued by all the resources their wealth can command.

Gentlemen, I have made my observations as your Honorary President. I trust there will be one still left to me to make as your friend, and that is, that I may observe you in days to come pursuing an honourable and successful career, with ever a warm corner in your heart for the old Society, the old teachers, and the old school.

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## A METHOD OF SECURING AND SUTURING THE BLADDER IN SUPRA-PUBIC CYSTOTOMY.\*

By A. ERNEST MAYLARD, B.S.Lond.,  
Extra Dispensary Surgeon to the Western Infirmary, Glasgow.

(*With Two Engravings.*)

THE practice of suturing the bladder after the high operation for stone or tumour has not hitherto found much favour in this country, owing, to a large extent, to the almost practical impossibility of properly coapting the edges of the vesical incision after the bladder has been opened, and the too frequent sequence of urinary extravasation when the endeavour has been made. Failures, therefore, having been frequent, all endeavours to bring about union in the vesical wound have fallen into disrepute; and thus a really worthy aim has suffered from faults which should be rightly attributed to the general method of operating.

Led by Sir Henry Thompson, British surgeons have usually adopted the method—after first exposing the viscus—of trans-

\* Read at the Surgical Section of the Medico-Chirurgical Society of Glasgow.



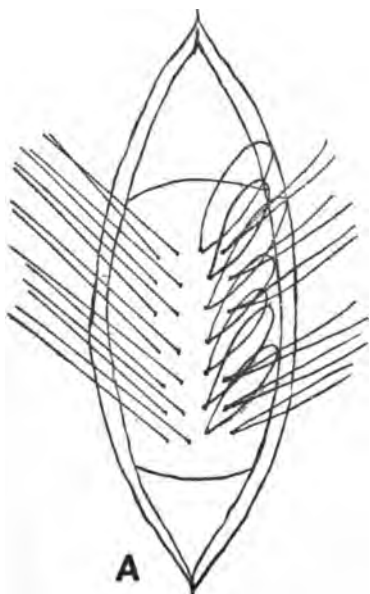
fixing the bladder with a small sharp hook, and then incising with a scalpel. The hook secures the bladder so as to prevent its parietes from receding into the pelvis after the evacuation of the fluid. The stone or tumour removed, one or two drainage tubes are introduced into the bladder, and the urine allowed freely to escape.

While thus in Great Britain the vesical suture has received but slight recognition, both in Germany and France the subject has been extensively discussed and the operation largely practised. Among German surgeons who have contributed, in one way or another to the subject, may be mentioned—Willy Meyer, Géza von Antal, Julliard, v. Bergmann, and Sonnenberg; and among French surgeons—Duchastelet, Bérard, Vincent, Monod, Dulles, and Bouley. In a recent number of the *Bristol Medical Journal*\* Mr. Greig Smith discusses the subject, and advocates the use of the suture, especially in young children. It may be incidentally remarked here that the present number of Langenbeck's *Archiv für Klinische Chirurgie* (vol. xxxvi, 1st part) contains an article by Dr. Edmund Assendelft, in which the subject of suturing the bladder is extensively discussed.

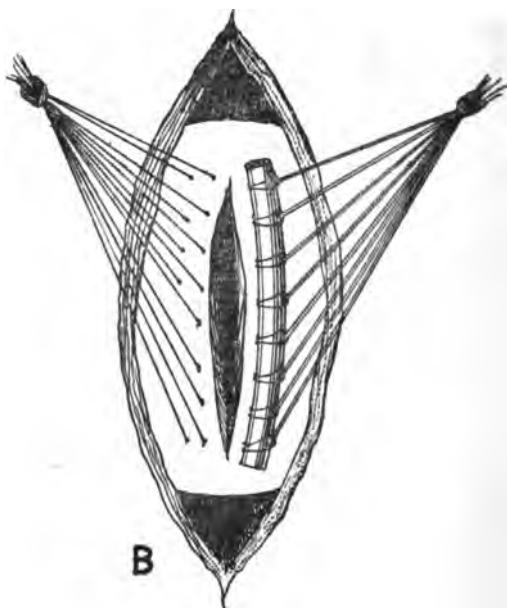
*Operation.*—As regards instruments, the only really extra requisite is a specially formed needle, made for me by Messrs. W. B. Hilliard & Sons of this city. It resembles an ordinary cleft palate needle mounted on a cylinder shaped handle, so as to permit of easy rotation between the thumb and fingers. The needle proper forms about a half circle whose diameter is a quarter of an inch, so that the tissue taken up by the suture will measure about that amount in width. About four yards of chromic gut are required. This should be cut in two, and the needle threaded from its *concave* side with one-half.

Until the bladder is exposed, the mode of operating is such as is usually adopted. Instead, now, of proceeding at once, as is usual, to open the viscus, the sutures are passed. An assistant takes charge of the long end of the suture while the surgeon rotates the needle through the muscular walls of the

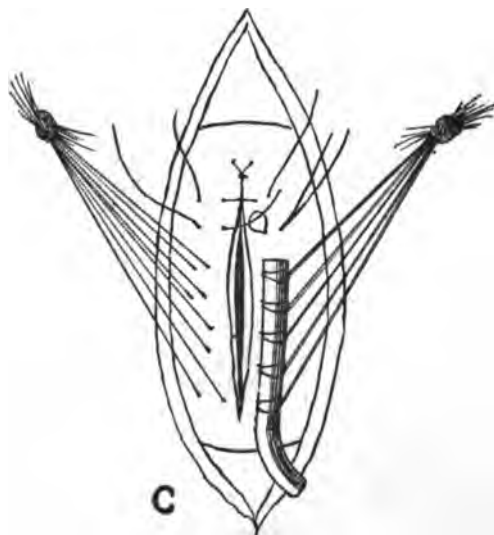
\* September, 1887.



**SHOWS STITCHES WHEN PASSED.**



**SHOWS BLADDER SECURED AND OPENED.**



**SHOWS PROCESS OF CLOSING BLADDER.**

bladder, commencing either above or below the position of his projected incision. This is a "loop" stitch such as is made in the operation for cleft palate and is to subserve a similar function. In withdrawing the needle, the gut should always be pulled from its *convex* side, thereby leaving the needle on the long end, and, therefore, always threaded. It should be a foot long, and when passed, given into the charge of an assistant. The needle is again introduced opposite the "loop" stitch at an interval of from one-quarter to one-third of an inch, and this time only a "single" stitch is passed, about a foot in length. The assistant takes charge of this stitch also. A series of six or eight similar stitches are passed on each side at intervals of about a quarter of an inch, as shown in the diagram A. The ends of the "single" stitches are tied together. The "loop" stitches have to be differently treated. Each two ends is passed through its corresponding loop, and before slipping tight a medium sized ordinary drainage tube is run in. This allows of the loops being easily loosened when subsequently required. The loose ends are tied together as on the opposite side, and each bundle held by an assistant, as shown in the diagram B. The bladder is now firmly secured, and the surgeon proceeds to open. A scalpel is introduced in the median line, about midway between the two extremities of the suture series and enlarged upwards and downwards by a pair of scissors bent at an angle. The primary opening should be large enough to admit of the index finger being introduced for diagnostic purposes. Should it be found that the stone or tumour is likely to require a larger incision than the existing series of sutures will cover, additional ones should be added before proceeding farther. It is important that the incision should not extend beyond the end sutures. The necessary length of incision completed by snipping with the scissors, and the stone or tumour removed, the vesical wound is united. This is accomplished by cutting off on each side one pair of sutures from their respective common knots, slipping the inner end of the "single" suture through its opposing "loop"—which has been disengaged from the india-rubber tube—and then, as in cleft palate, pulling the "single" suture through. This is then tied, and by so doing the mucous membrane of the bladder is turned in, while the fibro-muscular surfaces are brought into close apposition. The same process is repeated with the remaining stitches,\* as shown in the diagram C. They

\* Rather than to withdraw the tube, as shown in the diagram, it may be found easier just to snip off, with a pair of scissors, a piece as each loop is loosened.

are all then pulled slightly forward to see whether there is an interval where it would be wise to insert an additional superficial stitch. If the wound appears perfectly secure the ends are cut off close and the abdominal wound finally dealt with. With regard to the treatment of this latter no definite rule can be laid down. If the patient be young, the urine healthy, and the vesical wound small, there can be no objection to its complete coaptation; but under less favourable circumstances it would be wiser to insure free drainage. In all cases strict antiseptic dressings would be adopted. With regard to the use of the catheter, much difference of opinion exists. It appears in many cases to have been the cause of extravasation. Becoming blocked, the tension has become so great in the bladder that oozing has taken place through the vesical wound. Careful watching, however, should prevent this mishap, and if with certainty guarded against, its use is more likely to be beneficial than otherwise.

*Remarks.*—When efficiently carried out there is no doubt as to the perfectly complete occlusion of the vesical wound by the above described method of suturing. I have tried distending bladders in the *post-mortem* room, and find that great tension—much greater I venture to think than can take place in life—is required before oozing takes place. The important points appear to be (1) that the stitches should be close together, and (2) that the incision should not extend at either end beyond the terminal stitch. Still further, Maximoff's suggestion should be followed: that is, the mucous membrane should not be included in the passing of the stitches. It is a little difficult to be sure of this, and I venture to think, from my *post-mortem* experiments, that it would be wiser to err in taking up a little of the mucous membrane rather than too little of the muscular substance. It is for this reason that I have suggested chromic gut in preference to carbolised silk which has been mostly used. The objections to taking up the mucous membrane are the fear of concretions forming on the stitches, and the stitches themselves forming tracks for the percolation of urine. Chromic gut, I fancy, is less likely to cause these evils. It swells and eventually becomes absorbed—at least that part embedded in the tissues—while the particles in the bladder may be dislodged and carried away in the urine. The danger of taking up too little muscular tissue is, that the coaptation does not form a secure barrier against the results of tension. Plain carbolised gut was at one time much used, and not a few failures recorded have been due to the too rapid solution of the sutures. While

we are using a method of stitching resembling much that of Lembert, we are dealing with fibrous and not serous surfaces. Union is effected in a few hours after coapting the latter, but it requires days for the former; nevertheless, as has been amply shown by Vincent's experiments on dogs, if all goes well, the union soon becomes quite secure.

That even very bad cases will do well, when the bladder wound is efficiently sutured, was forcibly shown by a case reported by Dr. Géza von Antal, at the Berlin Surgical Congress,\* whose patient had stinking urine. The method adopted by this surgeon was to insert an india-rubber bladder into the vesical cavity, after the latter had been emptied of its contents; distend the same and with a scalpel pare the edges of the wound obliquely. These oblique raw surfaces were brought together and sutured.

The method which I have ventured to bring forward resembles more that originally described by Bérard than any of the other numerous kinds of sutures used. It differs, however, from Bérard's in many details which I need hardly discuss. I confess I was ignorant of Bérard's method, until I quite accidentally came across it a few days back, and can only describe my own as a modification of his.

I may say in conclusion, that the almost universal opinion of writers on the subject is, that some form of a Lembert suture is the only reliable one to use. Whether this be applied as proposed by Duchastelet† with a specially devised *cystorrhaph*, or as indicated by Dr. Géza von Antal, or in the manner I have described, matters little, so long as in every case the vesical edges are properly coapted. One advantage which I think the method described specially claims is, that even supposing it be deemed by the operator unadvisable to completely close the bladder, he can at least materially lessen the size of the incision without interfering with efficient drainage, and by so doing hasten the process of healing.

\* *Verhandlungen der Deutschen Gesellschaft für Chirurgie*, 1886.

† *Revue de Chirurgie*, III, 1883, p. 104.

## CASE OF MYXŒDEMA.

BY DR. PATON, GREENOCK.

*(Read before the Greenock Medical Society.)*

CASES of myxœdema are somewhat rare, and not much, so far as I am aware, has been written on the subject in Scotland, the chief information we have on the subject emanating from London. A case having come under my observation, I thought it might be interesting to bring it before the Society.

The first cases which I find published as cases of myxœdema are by Dr. Ord and Sir Dyce Duckworth in the *Lancet* of 1879. But some time previous to this date, in 1873 I think, Sir W. Gull had described the same affection under the heading of "A Cretinoid Affection occurring in Middle-Aged Women." Dr. Ord was the originator of the term Myxœdema, from the fact that mucin was found so extensively in the tissues, or rather cutaneous structures. The symptomatology recorded in these cases was briefly as follows:—Swelling, especially noticeable about the eyelids, and thickened lips; slowness of speech; slow, staggering walk; swelling of the dorsum of the hands, giving them what has been called "a spade-like appearance;" skin having a puffy, waxy appearance, dry, rough, and translucent; lethargy; mental irritability, defective memory, and in advanced cases mental aberration; subnormal temperature, in one case so low as to range between 90° and 92°; urine containing no albumen, or merely a trace, no sugar; absence of thyroid gland, or so small as not to be detected. Such is an epitome of the symptoms of the first published cases. I may state that at this time the disease was thought to be entirely confined to women, Sir Dyce Duckworth stating that it had never been observed in males, but only in adult females. Now, of course we know this to be erroneous—so much so, that Sir Andrew Clark, in 1881, stated that he had seen the disease more frequently in men, but his experience in this respect seems to be isolated. Dr. Inglis records two cases in the *Lancet* of 1880. The symptoms which he observed, besides those already mentioned, were coldness of the surface of the body to touch, no reflex contractions on tickling the soles of the feet, no atrophy of the muscles, but the grasp of the hand was feeble, chiefly from inability to flex the fingers. One of these cases was that of a male. In one of the cases reported by Sir Dyce Duckworth, moles were found on the face, scalp,



and left shoulder, acquired and not congenital; and in connection with this he points out the concurrence of defective mental condition with *Molluscum Fibrosum* as suggestive of the association of moles with the impaired mental powers in myxœdema. Dr. Semon also reports a case in which, in addition to the typical symptoms, there was a marked falling off of hairs from all parts of the body, and also a brittle condition of the nails. The absence or atrophied condition of the thyroid gland will be more particularly referred to again. Such, briefly, is the symptomatology of myxœdema.

The etiology and pathology of this disease are still somewhat obscure, and different theories have been advanced to account for the phenomena which arise during its progress.

At first the general consensus of opinion as to the etiology seemed to be that it arose from some lesion in the central nervous system, and not peripheral as some had supposed, as it was found the nervous symptoms were characteristically constant, and generally could be shown to precede the cedematous symptoms. In a number of cases the symptoms have apparently developed after mental shock or anxiety, and on this ground the frequency of the disease in multiparous women has been to a certain extent accounted for. In connection with this same fact, Dr. Hadden thought that the sympathetic was primarily at fault, but latterly he found this theory untenable, as he had examined the cervical sympathetic with negative results. Dr. Mahomed thought that this affection was only another phase of chronic Bright's disease owing to the heightened arterial tension, and the fact of a case of his having improved under the nitro-glycerine treatment. A discussion took place at the Clinical Society of London in December, 1883, when a somewhat new phase was put on the etiology of this disease. Although from the time of Sir W. Gull's original communication, the absence or diminished size of the thyroid gland had been observed as a comparatively constant fact, still the connection between myxœdematous symptoms, and this absence of the thyroid did not seem to be so fully appreciated, until Dr. Felix Semon communicated the after results of Professor Kocher's (of Berne) cases of extirpation of the gland, which were very remarkable. He was able to trace 18 patients on whom he had performed total excision of the gland. In 16 of these cases the train or gradual sequence of symptoms which he describes under the name of *cachexia strumipriva* (not knowing of myxœdema at this time) exactly corresponded with those of myxœdema; and in one of the other 2 cases a small accessory thyroid gland had undergone

a hypertrophic change, and in the other there was a recurrence of the goitre.

I would only refer further to this interesting subject, by calling your attention to the *résumé* of Professor Virchow's paper, in the *British Medical Journal* of 19th March, 1887, in which the great pathologist inclines to the theory, that in the loss of the thyroid gland is to be found the essential cause of the train of symptoms observed in myxœdema.

The following is a history of the case which has come under my care. I may mention that the patient was seen by Professor Gairdner, and he confirmed the diagnosis.

Mrs. B., æt. 47 years, married, has had six children, one of whom died of diphtheria at the age of seven years. All the others have been very healthy, the youngest being now seven years of age. She had two miscarriages, one at the third month, the other somewhat earlier. Her confinements were all natural, and she made good recoveries. About five years ago she had a very severe attack of acute diarrhœa. With these exceptions, she has always enjoyed good health up to the present illness.

Her father died, at the age of 58 years, of paralysis, having had two or three shocks. Her mother died at the age of 76 years of old age. She had six sisters and three brothers. One of her sisters died of consumption, at the age of 48 years. Another sister died of inflammation of the lining membrane of the bowels (probably tubercular peritonitis), over 20 years of age. Another sister died of consumption of the bowels, also over 20 years of age. The remaining members of the family are all healthy. She ceased menstruating two years ago. She was from infancy of an exceeding pale complexion, but notwithstanding was looked upon as the healthiest and strongest in the family, although of a nervous temperament.

In the last week of August of last year, she was visiting some friends, and had the misfortune to fall down stairs, but did not receive any serious injury. At this time she had also a good deal of mental annoyance. From this period she dates the commencement of her illness. The first symptom which attracted her attention was that of stiffness in undoing her boots, not so much from the swelling which was present, but, as she expressed it, from a feeling of hardness. The face was swollen, especially about the eyelids. The swelling is not strictly constant, she affirms, but comes and goes somewhat. The skin is rather rough, dry, and translucent, that of the upper eyelids having the appearance as if it had been blistered with cantharides. The hands have the appearance of being

broadened out and swollen, although, she says, she had always large hands. They feel stiff when using them, and her grasp, though fairly good, is not so firm as one would expect. When knitting, she requires to stop frequently, owing to the pain produced. In cold weather she has the feeling of cold water running all through her. She complains of a peculiar husky feeling in the throat, sometimes causing a choking sensation, the voice even going away to a great extent. She cannot sing, and now never attempts it even in church. She experiences a difficulty in speaking, and has observed the difference in the tone of her voice. Her speech is slow and deliberate, and, after speaking a long sentence in one breath, the inspiration following is rough and somewhat croupy in character. After engaging in conversation for any length of time, she gets tired, her speech becomes thick, she takes longer to get her words, and feels as if the tongue was swollen—in fact, too big for her mouth; she has actually bitten it. Her memory is not so acute or accurate as formerly. She can converse intelligently, but cannot remember things for any length of time; and if the conversation be carried on for long, her face becomes flushed, as if from the forced exertion. She cannot read any book requiring much concentration of thought; it must be some simple continued story, such as (her husband tells me) *John Halifax, Gentleman*, and, even with this, feels herself get somewhat confused if she reads too long. When speaking of her mental condition, I may mention that one of her relatives, who had not seen her for some time, asked me if I did not consider her mind affected, as she seemed to her to have a vacant kind of look she had not observed before. I merely mention this to show the connection between the appearance of myxœdematous cases and the "cretinoid affection" of Sir W. Gull as noticed by a non-skilled observer. She complains of constant noise in the head, which has been increasing of late. There is considerable swelling in the submaxillary region on both sides, and she has had to get her jacket widened to fit her neck, the feeling being as if the swelling stretched round to the back of the neck. In February of this year an oozing of dark fluid, which she considers mixed with blood, came from the mouth during night, staining the pillows. This has continued more or less ever since, but has never taken place during the day, with one exception—while washing herself.

When out alone walking she was often ashamed of herself walking so unevenly with a staggering gait, that, as she expresses herself, people looking at her would think she had

been having too much of what was not good for her. In order to prevent this she would map out an imaginary line on the street along which she would walk for the purpose of keeping her straight. She has frequently a feeling as if she would fall, and, in fact, really did fall once in her own house.

This patient has an exceedingly pale, waxy, anæmic appearance. One casually looking at her would suspect either chronic Bright's disease or possibly pernicious anæmia. I have examined the urine frequently, and have never found a trace of albumen or sugar. It is rather pale in colour, slightly acid in reaction, having a specific gravity of from 1014 to 1016. She takes her food well and sleeps well; in fact, is inclined to sleep too much. The knee-jerk is present in a very limited degree. Tickling the soles of her feet produces no reflex movement. In fact, one would say she was in a chronic lethargic condition, both mentally and bodily.

Her temperature (which I showed the husband how to take) is characteristic and interesting. It is always sub-normal, once not rising even to 95°, and, from the continued series of observations taken, almost invariably higher in the morning.

Dr. Cluckie examined ophthalmoscopically for me, but could detect nothing abnormal, with the exception of possibly a slight dilatation of the choroidal vessels. I cannot feel anything of the thyroid gland. She is able to go about her usual household duties, but not with the same life as formerly.

## PUERPERAL SEPTICÆMIA: ITS CAUSES, AND THE MEANS TO BE ADOPTED FOR ITS PREVENTION.

By J. ANDERSON ROBERTSON, M.A., M.B.

(*Read before the Medico-Chirurgical Society, 4th November, 1887.*)

I must confess, Gentlemen, that it was not without considerable misgivings that I affixed the name "puerperal septicæmia" to the short paper I am now about to read. In doing so, however, I merely followed the example given by one of our most distinguished British Obstetricians, Dr. W. S. Playfair, who, in thus naming the disease, has certainly come much nearer the truth than do those writers who still adhere to the old title of "puerperal fever." Perhaps the best name which, in the present state of our knowledge, can be applied to the disease is "puerperal sepsis." This will at least com-

prehend all sources of infection, whether autogenetic or heterogenetic; and will include the various diseases which arise either from putrefaction or fermentation—viz., sapræmia, septicæmia, and pyæmia. The old and still much used name of “puerperal fever,” ought to be done away with altogether, as it was founded upon an entire misconception of the disease. This misconception arose from the belief that puerperal sepsis is a specific fever, which attacks women during the puerperium, independently of any local affection in the organs of generation. The disease was supposed to occur in epidemics just as any zymotic fever might do, and to be altogether independent of and beyond the care or cleanliness of the physician, or of the surroundings of the patient. This belief seems, indeed, not yet to have quite died out, as I heard not long ago, an old practitioner give as his opinion, that the reason why “puerperal fever” (as he called it) is much less common now than formerly, is that the atmospheric conditions of this country have undergone a great change during the past 35 or 40 years. I asked him if cleanliness and improved sanitation had nothing to do with the disease; and he merely smiled and said “no!”

Among the ancients, so far as one can learn, the disease does not appear to have attracted much attention. It seems to have been looked upon as one of the accidents to which puerperal women were unavoidably exposed; and many deeply religious persons evidently regard it as a fulfilment of the curse pronounced upon Eve—a feeling akin, no doubt, to that which causes many women still to hold up their hands in horror, when one proposes to alleviate the pains of labour by the administration of an anæsthetic. Those who did not hold this quasi-religious view, took, however, no brighter one; for in their opinion, the death of a certain proportion of women in childbed was just as unpreventable as was that of a certain percentage of patients after accidents, major operations, &c. Indeed it is only of late years that the medical profession has been awakened to the realisation of the fact, that *all* cases of puerperal sepsis *are* preventable: that upon the careful attention to cleanliness, on the part of the medical and other attendants, depends to a very large extent the recovery or non-recovery of women from labour; in a word, that in the hands of the profession are the issues of life and death to the mothers of the race.

Until the middle of the seventeenth century puerperal sepsis, although very common, seems to have attracted little notice. In 1646, however, so great was the death-rate among

puerperal women in Paris that the attention of the medical profession throughout Europe was drawn to the subject. In February of that year 70 women were confined in the Hotel Dieu, and all died. Nor was the fatality confined to hospitals, for in private practice, though much less deadly, still large numbers of mothers fell victims to the disease. During the winters of 1774, 1775, and 1776 the death-rate from puerperal sepsis in the various maternity hospitals of the French capital averaged over 60 per cent.

After attention had been attracted to the frightful fatality of the puerperium, it soon came to light that Paris was not singular in this unfortunate condition of affairs, but that Vienna, Berlin, London, and most other large cities where lying-in hospitals existed, were from time to time visited with what appeared to be unpreventable epidemics of this fatal disease. Again and again were individual hospitals closed for a time, until it became the almost prevailing opinion of the profession that all such institutions should be done away with for ever. Unfortunately the state of affairs which obtained in private dwellings was not much better. In the practice of every well known obstetrician we have a repetition of the same sad story of so-called fever and death. So late, indeed, as 1842 we have, in the *London Medical Gazette*, a then well known lecturer on midwifery—I mean Dr. Robert Lee, of St. George's Hospital—writing as follows:—"The disease, has been observed by me to prevail during the last fifteen years during spring, summer, autumn, and winter, though most frequently in the last, not only in the British Lying-in Hospital, the lying-in wards of the St. Marylebone, St. James, and other parochial infirmaries, but in every district of London and the surrounding country, and in all the different ranks of life." The disease has generally arisen, like inflammation of the bowels and lungs and other viscera, without any assignable cause; where the process of parturition has been completed in the most natural manner; where nothing could be discovered peculiar in the constitution of the atmosphere; and where typhus fever, scarlet fever, erysipelas, and other contagious and epidemic disorders were not prevailing to an unusual extent. But it is an opinion which has long prevailed that the uterine inflammation of puerperal women is of an erysipelatous nature, and that it is excited in some cases by contagion or depends upon a vitiated state of the atmosphere, like hospital gangrene, and may be communicated from one patient to another by the nurse and medical practitioner."

The literature of this subject is intensely interesting, and

shows how utterly at a loss mediciners were to account for the disease. In this short paper I merely beg to call your attention to it. Before leaving it, however, I shall bring under your notice a remarkable paper contributed by John Hunter in 1793 to "The Transactions of a Society for the Improvement of the Medical and Chirurgical Knowledge." In this paper Hunter describes very graphically (and, so far as I know, for the first time in history) inflammation of the venous system. "I have found" he says "in all violent inflammations of the cellular membrane, whether spontaneous or in consequence of accident, as in compound fractures, or of surgical operations, as in the removal of an extremity, that the coats of the larger veins passing through the inflamed part become also considerably inflamed, and that their inner surface takes on the adhesive, suppurative, and ulcerative inflammation. I have found in many places of the veins adhesions, in others matter, and in others ulceration. Under such circumstances the veins would have abscesses formed in them, if the matter did not find in many cases an easy passage to the heart, along with the circulating blood, so as to prevent the accumulation of pus; but this ready passage of the matter into the common circulation does not always happen. It is in some cases prevented by the adhesive inflammation taking place in the vein between the place of suppuration and the heart, so that an abscess is formed, as will be further observed: where the inflammation is most violent there we find the vein most inflamed; there, also, after suppuration, we find the purest pus, and as we trace the vessels from this part either farther from or nearer to the heart, we find the pus more and more mixed with blood, and having more and more of the coagulated parts of the blood in it. As these appearances are only to be seen in dead bodies, they cannot be described but from thence. I have found them in the bodies of those who have died from amputation, compound fractures, and mortification." This remarkable paper, taken in conjunction with his description of puerperal peritonitis, has caused me to believe that Hunter must have had in his mind the similarity of conditions which exist in the site of the recently removed placenta and in raw surfaces resulting from wounds or incisions. In his paper on the latter subject Hunter says:—"Peritoneal inflammation happens a few days after child-birth, which has been called puerperal fever. The fever is only a sympathetic affection in consequence of the inflammation of the peritoneum, although this last has been thought only a symptom of fever." Laennec while

clearly recognising the formation of abscesses in various organs after operation, attributes it wholly to the presence of pus in the blood—true pyæmia (*πυον αἷμα*)—though not what we now strictly understand by the term pyæmia. “An additional consequence,” he says, “of the presence of too much pus in the blood is the production of inflammation in different organs, and especially in the lungs, which runs rapidly into suppuration. It is from this circumstance that the subjects of surgical operations, and those labouring under extensive suppurations, are frequently cut off by peripneumonies.”

Since the discovery of the cause of inflammation or putrefaction in animal tissue, or wound exudations by Pasteur, the exact similarity in conditions which exist between a puerperal woman and a person who is the subject of a surgical operation has gradually come to be recognised; and it is now an absolute certainty that traumatic or surgical sepsis and puerperal sepsis are one and the same thing. Whether the symptoms of fever which gave rise to the old name be evoked by the presence in the blood of one of the alkaloidal products of decomposition, *i. e.*, *sapraemia*; or be due to the general infection of the whole system by a rapidly increasing poison, *i. e.*, *septicæmia*; or by the general infection of the system by a slowly increasing poison, with the production of secondary abscesses in various organs of the body, *i. e.*, *pycæmia*; all are included under the name *puerperal sepsis*; and all the disorders arising from that sepsis depend upon putrefying or septic fluids in a wound gaining admission to the general circulation. Blood poisoning *may*, it is true, occur quite independently of the presence of a wound, but such cases are very exceptional, and do not apply to childbirth, where, even if no wounds of perineum or cervix be present, we have always the exposed raw surface previously occupied by the placenta.

The distinction between so-called *autogenetic* and *heterogenetic* infection, although mentioned at the beginning of my paper, will not, I venture to think, hold good. I doubt very greatly whether there be such a thing as autogenetic infection, although much has been written and said about it. Many old writers recognised autogenesis as the most common means of infection, as will be seen from the following translation of Haller, lib. xxix, part of section V:—

“The majority of experts are of opinion that more women have died through clumsy removal of the placenta than from



difficult labour. . . . They (*i. e.*, the experts) have a dread lest the retained placenta, as too often happens (and that if only a small fragment has been left), should produce very rapid gangrene. By this means putrid pus is either poured back into the connecting vessels of the uterus, which saturates the mother with a material eminently calculated to produce fevers of the most deadly kind, or by simple contact brings about gangrene of the uterus itself. Even if afterwards the portion of placenta be expelled, the woman is likely to die; for it is hardly to be expected that the uterus can escape infection when the secundines are putrid."

The frequent epidemics of so-called "puerperal fever" caused many inquirers to look beyond the autogenetic theory, and although a goodly number came to the conclusion that it was no more heterogenetic than pleuritis, nephritis, or any other inflammatory disease, the majority acted as if it were; "not," as one of them says, "from being convinced of its necessity, but for the satisfaction of my own mind" (Hey, of Leeds). Other inquirers, however, came to the conclusion that autogenesis would not explain all cases; and Gordon, of Aberdeen, acknowledges that he was himself the means of conveying the infection to a great number of women.

From what I have already said, it may readily be inferred that I have no belief in the theory of *autogenesis*, or self-infection. It naturally follows that I am of opinion that every case of puerperal sepsis is preventable, just as is every case of surgical sepsis. In puerperal sepsis too, as in surgical, any micro-organism which is capable of multiplying and of generating poisonous products in the blood, or even any poisonous product itself of those micro-organisms will be sufficient to produce what was formerly known as puerperal fever, with its varying symptoms.

In the case of poisoning by the alkaloidal product, the nature of the symptoms and amount of disturbance will be directly as the dose of the poison, and the physical condition at the time being of the patient. In the case of the entrance of the living and multiplying micro-organism itself, the symptoms and result will depend entirely upon the physical condition of the patient, the absorption of an infinitesimally minute amount of the poison being sufficient to produce the most violent symptoms and fatal result.

The causes of puerperal sepsis thus come to resolve themselves briefly into:—

1st. *Intrinsically*—The condition of the woman herself at the puerperal period; and

2nd. *Extrinsically*—The circumstances which surround the woman at that period.

With regard to the former set of causes, one has only to consider for a moment the condition of a woman's blood at the time of her confinement to at once realise how vulnerable she is to septic influences. In addition to the waste products from her own tissues, she has coursing in her blood those from the tissues of her offspring; and this, too, at a time when her blood has been to an extreme degree impoverished by affording pabulum for the nourishment and rapid growth of the child. At the time of delivery her powers of resistance are further reduced by loss of blood, powerful, and often protracted, muscular exertion, and intense physical, and not unfrequently, mental anguish. After delivery the quality of the blood is still further reduced by the addition to it of the effete matters which are poured into it during involution of the uterus, as well as by the extraction from it of material for the nourishment of the child after birth.

Were women to live natural lives, with plenty of out-of-door exercise, and the free circulation and hearty appetite which such exercise engenders, these natural processes would undoubtedly affect them much less than they do, since the free circulation with its rapid oxygenation of the blood would readily dispose of the effete material, and the hearty appetite would insure the taking of plenty of food for the efficient nourishing of both parent and child. This exercise, too, would so tend to the normal development of women's bodies—*sana pelvis in corpore sano*—that most of the suffering during labour and consequent prostration would be done away with. As one generally sees it, parturition is not at all what it ought to be, a purely natural process, begun and ended by nature with little suffering, and less risk to the mother; but it is what women have to a very large extent themselves made it, by their insane practices of tight lacing, &c., their want of exercise in the fresh air, and their luxuriousness—a period of great suffering and greater risks. Nor are our men any less to blame in this matter. For so long as young men will admire wasp-like waists; husbands fail to insist on their wives taking exercise out-of-doors, or at least, fail to give them the opportunities of so doing; fathers pander to the namby-pambyism of the present age, which tends to sacrifice the health of their daughters to the customs of fashion, or the wrongly-called "higher education of women," so long will women be distorted and unhealthy, and parturition be difficult and dangerous.

Under the existing conditions of our social life, the blood of parturient women forms the best possible nidus and pabulum for the preservation and rapid multiplication of any micro-organisms which may obtain admission thereto, while the prostration succeeding labour takes away all power of resistance against their influences. When we come to add to these conditions the fact that, after the removal of the placenta, there exists a considerable raw exposed surface, to which septic matter is readily conveyed, and through which it readily reaches the general circulation, the absolute necessity for the prevention of such morbid matter from reaching this surface can at once be understood. This consideration naturally carries us to the second set of causes—viz., those which surround the woman.

These may be briefly summed up as the hygienic conditions of the locality and house in which the woman resides, and of the medical and other attendants who are brought into contact with her. If, in short, septic material be conveyed to the placental site or to any wound which may have been accidentally incurred, puerperal sepsis is sure to be set up, unless, indeed, it should happily chance to be the case that the woman has sufficient resisting force left to render her invulnerable to the virus.

The means by which puerperal sepsis is to be prevented, although simple enough to enumerate, are not so easily carried into practice. Over those causes which are at work in the woman herself prior to the puerperium we have frequently very little control, inasmuch as pregnancy is often far advanced before we are consulted. Still, however near confinement may be, we may yet do something; and, when consulted at an early period, we may do much to render a woman invulnerable by the adoption of such simple prophylactic measures as the following:—

1. Insisting upon a fair amount of outdoor exercise every day.
2. Attention to the condition of the bowels, taking care that a passage is obtained every day.
3. The taking of sufficient suitable nourishment, with the avoidance, as far as possible, of such hot irritating drinks as tea and coffee.
4. Most rigid attention to cleanliness—the frequent sponging or bathing of the whole surface of the body, so as to stimulate as much as possible the excretion of effete matters through the skin.
5. The prevention of anything like tight lacing, by which

all the pelvic organs become congested, and are thus made more vulnerable to septic influences.

By these and every other means whereby the mother's general condition can be improved, much may be done to render inoperative any virus which may possibly reach the circulation after labour. Of course, with defects of condition or constitution which have resulted from fashion or heredity, we cannot do much primarily, although, by the honest expression of our opinions, we may influence in no small measure the rising generation of womankind.

The extrinsic causes of puerperal sepsis are so far under the control of the medical attendant that only where the sanitary condition of the patient's house is bad, or where his orders are disobeyed, ought such a thing to be possible. Indeed, I will go so far as to state that scrupulous attention to rigid cleanliness on the part of the physician, backed up by obedience on the part of the nurse and members of the household, will in most instances neutralise and render inoperative the intrinsic causes, by preventing access of any infectious virus, without which they are altogether powerless to produce puerperal sepsis, however much they may otherwise complicate or retard recovery.

There may be some gentlemen present here who, like Tait and Bantock, profess to ignore the antiseptic treatment, but to such I would say that, if, like these distinguished surgeons, they become so imbued with the spirit of Listerism that perhaps unwittingly they carry it out in its minutest details, by strict attention to rigid cleanliness, they may well be excused the observance of the letter of the law. It is only, I venture to maintain, by thorough-going observance of this law of cleanliness—call it Listerism, antisepticism, or any name you like—that we can hope to prevent the occurrence, either in private or hospital practice, of the altogether too common scourge, puerperal sepsis. "Cleanliness is next to godliness;" so says the proverb. It ought to have said "is *part of* godliness." And as healing the sick and combating with death are divine occupations, they are most likely in our hands to be carried on successfully when united with the divine attribute of cleanliness.

At the risk of being tedious, and of repeating what you yourselves know as well as I do, I shall briefly enumerate a few of the methods by which the law of cleanliness may be applied to the subject in hand:—

1. Find out shortly before the confinement is due whether the nurse engaged is attending any infectious case, or has

in her home any case of an infectious nature; and if so, refuse to have her.

2. Inform the patient beforehand that you wish the lying-in-room to be devoid of carpet and curtains, and to have as little furniture in it as is compatible with comfort. Let the bedstead be iron if possible, and be very careful to see that there are no articles of clothing, furniture, &c., stowed away under it. If there be a choice of rooms, select one with a bright outlook; and while your patient is confined to it, let plenty of light into it, and as much fresh air as is consistent with safety to mother and babe.

3. If called to a midwifery case while in attendance upon any infectious cases, never go straight from one of the latter to the former. It is one's bounden duty to first go home, and there scrub oneself from head to feet and change every article of clothing. This should always be done, however urgent the call to the midwifery case may be.

4. When at the patient's bedside, care ought to be taken that everything—fingers, sponges, instruments, &c., which may have to be brought into contact with the genital tract—be dipped in an antiseptic solution or smeared with an antiseptic lubricant. I use a lubricant prepared by Messrs. Currie & Sons, and find it very efficient. A free enema during the first stage, with the subsequent careful washing of the parts, is good practice.

5. Everything possible should be done to conserve the lying-in woman's strength, as, *e. g.*, the prevention of greatly protracted labours by judicious assistance, the giving of chloroform, &c.

6. After delivery, the thorough expulsion of the secundines is to be carefully carried out. The best means I know for effecting this, and at the same time for causing uterine contractions, is to turn the mother on her back, put the babe to the breast, and with both hands firmly grasping the uterus, follow it downwards and backwards into the pelvis.

7. See that all blood-stained articles of body or bed-clothing be removed.

8. Allow the woman from the first plenty of suitable nourishment.

9. Use a vaginal douche of 1-4000 perchloride of mercury solution every day; and encourage the patient to change her position fairly frequently—from back to either side, and *vice versa*. The object of this is to dislodge clots, stagnating discharge, &c.

10. Allow no one to enter the room excepting members of

the household for at least 4 days after delivery. There is no knowing what germs of infection an outsider may carry in.

11. Always take care that such articles as bed-pans, drinking-cups, &c., which may be borrowed from kind neighbours, or from a chemist, who lets such articles out on hire, be carefully washed in a strong antiseptic solution, before the patient is permitted to use them.

12. Keep your finger nails scrupulously clean.

The carrying out of these and other minor details will involve, no doubt, a little extra trouble; but this will be more than counterbalanced by one's having the satisfaction of knowing that he has done everything in his power for the welfare of his patient. Among the poor, it is impossible to have things as one would wish them to be; but if every practitioner will do his best, under existing circumstances, to attain to the law of perfect cleanliness, he will often succeed in preventing puerperal sepsis, however insanitary the home or unpromising the surroundings.

To my mind nothing is more sad than the death of a woman in childbed. It means a blank in the lives of child and husband which nothing on this side of the grave can ever adequately fill up. If such blanks can be prevented from occurring (as I am convinced they often can) by an attempt on our part to fulfil the law of cleanliness, don't you think, Gentlemen, that the attempt is worth making?

[The discussion on this paper was adjourned, and will be taken up on Friday, 9th December.]

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## CURRENT TOPICS.

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EAR HOSPITAL.—We learn that the course of instruction in the Ear Hospital is being largely taken advantage of this year. The course is being conducted, for the first time, in the evening, and this change seems to have met with success. As many as 100 students attended the first meeting, and 74 have enrolled their names for the course.

## REVIEWS.

*The Operative Surgery of Malignant Disease.* By HENRY T. BUTLIN, F.R.C.S., Assistant Surgeon and Demonstrator of Surgery, St. Bartholomew's Hospital. London: J. & A. Churchill. 1887.

THE modern development of surgical literature, which finds its outlet in the manifold and multifarious productions of isolated treatises upon every conceivable disease of every system or part of the human body, has found yet further extension in the shape of a work specifically devoted to malignant disease and its operative treatment. It is, perhaps, a hopeful sign, as indicating the gradual development of our art, that this differentiation and extension is being at present so largely indulged in. Not only does it tend to produce a more exact and ample discussion of any particular subject, but subserves the perhaps still more important function of so concentrating attention upon it, that additions and corrections are the more readily made, and true progress thus enhanced. In this latter respect, the book before us is likely to contribute much, or, at least be the means of doing so in the future.

Nothing, perhaps, is more needed in the treatment of malignant disease than to know the remoter effects or the subsequent history of cases previously operated upon; to know, in other words, whether our treatment has been radical or simply palliative. Custom has so led to the performance of certain practices, which have become stereotyped almost as imperative duties, that we fail too frequently ever to question the treatment by a reference to the results. Routine practice is a hard groove to deviate from, but it becomes the easier to do so the more we turn from the practice to the results and allow them to be our guide. In making, therefore, the "results of operations" the centre of chief interest in his book, Mr. Butlin has attracted attention to a branch of the subject fraught not only with interest for the information actually conveyed but for the more important consideration of awakening a critical insight into the practice usually adopted. But before speaking further on this point, we may give some general idea of the plan upon which the book is constructed.

The term "malignant disease" is taken to include both sarcoma and carcinoma, but where possible, and where indeed there is a distinct difference between the clinical symptoms

and the usual course of the affection, the pathological distinction is drawn and each form of the affection independently discussed.

Every organ or part of the body subject to malignant disease, mostly as a primary centre, receives separate treatment, and a common method is adopted in discussing each. Thus the clinical symptoms are first dealt with, then the "methods of operation," thirdly, the "results of operation." This last is further subdivided into—(1.) Mortality due to operation. (2.) Cures due to operation. (3.) Are patients who are not cured relieved by operation? Lastly, "conclusions." It will thus be seen that the arrangement is an extremely serviceable one, enabling the surgeon to turn, at any time, to any particular section.

The most laborious portion of Mr. Butlin's work has been naturally that dealing with the results of operations upon the different organs and parts, involving as it does an extensive reference to statistics. Statistics as a rule do not form a very reliable basis for investigation, but the author appears as much alive to the fallacies as to the truths which they may supply. Thus, in the case of operations, due regard is always had to the use or non-use of antiseptics; and septic results have not been allowed to exercise any material influence upon the general merits of any particular operation.

It must be said, however, that this is a consideration not wholly without difficulty. For not only is it often impossible to determine whether or not the dressing of a wound has been carried out strictly antiseptically unless the details are accurately described, but even the author himself seems somewhat hazy upon what should or should not constitute Listerism. Thus, on page 360, in speaking of operating upon the breast, he observes, "although the Listerian dressing is not employed, it will be seen that great care is taken that the operation shall be performed antiseptically," and on page 343, in connection with ovariectomy, "the Listerian dressing of to-day is not the Listerian dressing of yesterday, and I am sure most surgeons would find it difficult at the present moment to describe exactly what is now meant by the Listerian method of operating." It seems strange at this late period of the day to be talking of the Listerian *method* of dressing, when indeed Listerism has become entirely submerged in a broad principle, and not in any particular kind of wound application. The use of bichloride of mercury is as much Listerism as the use of carbolic acid, and these in whatever shape or form they may be applied. Both are employed



purely in regard to a principle; so that indeed it matters little what be the agents used, so long as they are used with the sole object of combating the various causes of wound infection. Antiseptic surgery and Listerian dressings can now no longer be aught else than synonymous expressions.

To resume, however, our examination of the various statistics given, some very striking results are brought out in connection with affections of certain parts. Thus, in connection with the breast, much encouragement is derived from the comparatively large number of cures which appears to have been effected. And here it may be incidentally remarked that no case is considered a cure unless a period of three years has elapsed without any further symptoms of development of the disease showing themselves since the time of operation. One series of statistics gives, of 98 patients operated upon, 19 complete cures—that is, that had passed the three years limit; another series, 11 instances of cures in a total of 144 cases; and the author himself believes that a percentage of 12 to 15 may be regarded as approximately correct. While yet the percentage of cures is in reality small, it nevertheless is better than many a surgeon would be likely to expect. The tongue also affords better results than we are accustomed usually to look for. Thus, out of 10 cases operated upon by Sir James Paget, 3 were alive and well at periods respectively of  $3\frac{1}{2}$ , 6, and more than 6 years after the operation. In Kocher's series of 14 cases three were completely cured. No part of the body, however, compares so favourably as the lip. Of a series of 424 persons, no fewer than 160 had passed the three years limit—that is to say, a percentage of successes a fraction over 38. Next to the lip comes the penis, where in one series of cases a percentage of cases of rather more than 18 was obtained, while in another a percentage of more than 27. Supra-vaginal amputation of the uterus also presents a series of very good results. Thus, in one series of 100 cases, 22 patients lived without recurrence for at least three years after the operation. From these few references to some of the carefully selected statistics given in connection with the parts above mentioned, it will be seen how much encouragement may be derived from the operative treatment of malignant disease in at least these particular regions. If Mr. Butlin leaves little to be questioned in the fair and reasonable way in which he has dealt with his statistics, it cannot be said that he occupies quite the same irreproachable position in relation to the views which he holds on many points in reference to the subject

of operation. With what varied feelings, almost amounting to resentment, will the following passage be read by many a surgeon:—"Because the cancers of certain organs and tissues tend to affect the lymphatic glands at an earlier or later period, it is a grave error, and to my mind a piece of rough and blundering surgery, to treat every individual case of cancer of those parts as if the glands were already affected." This statement, although occurring in the introductory part of the book, has reference presumably to the operation upon the breast and the removal of the axillary glands, for it is only in this region that such a practice is really systematically carried out. In turning to the section on the mamma itself, Mr. Butlin elaborates more fully his views on the subject, and takes the somewhat exceptional position of advising the removal of the axillary glands only if they can be felt or "there is fulness which does not amount to actual proof of enlargement of the glands." We say exceptional position, because most surgeons who have seriously considered the subject are inclined to the more non-exclusive practice of removing the axillary glands under any conditions. The reason for this practice has been the impossibility of determining whether or not the glands are infected in cases where there is no external indication of their being so. And this founded on the well observed pathological fact, that glands are often found diseased, and sometimes indeed considerably enlarged, when nothing could be detected previous to operation. Still further, that to remove the glands at the earliest possible stage is in all likelihood to prevent the subsequent systemic infection which would inevitably occur. To this practice, as stated above, Mr. Butlin is entirely opposed, and it must be said that from the source from whence he draws his conclusions, there is considerable force in his opposition. This particular source consists in a double series of statistics, taken apparently from reliable published papers. One series consists of 141 cases where the mamma alone was removed, and 170 where the glands in the axilla were also extirpated. In the first series there were 12 deaths from the operation and 19 complete cures. In the second series 39 deaths from the operation and 11 cures. But as 43 cases were lost sight of in the first series, and 26 in the second, the number of cures must be counted as 19 out of 98 in the former, and 11 out of 144 in the latter. Again, in the case of simple mammary extirpation 48 had recurrence of the disease *in situ*; 3 were dead or dying of affection of the axillary glands without

local recurrence; 3 appear to have died of dissemination of the disease without local recurrence. Thus it will be seen that while the major operation caused more than double the number of deaths, it effected less than half the number of cures. As Mr. Butlin points out, the number of deaths in both series is much larger than would occur at the present day, and also that the cases dealt with by the major operation were of a more extensive character than those where the minor was employed. But the really forcible argument which Mr. Butlin brings forward in comparing these statistics, is that out of the 98 cases of simple mammary extirpation, only 3 had recurrence in the axilla without recurrence in or near the scar. Assuming, continues Mr. Butlin, for the sake of argument, that these three patients had been spared the glandular disease which caused their death would "the saving of these three persons have been sufficient to justify the infliction of a very extensive and severe operation, wholly out of proportion to the extent of the disease, on more than 130 women?"

It cannot be denied that the facts shown above are very striking, and form a very feeble plea for running additional risks in operating. The only question we feel inclined to ask is whether there is really so much additional risk in clearing out the axilla? It is the experience of many surgeons who have frequently practised the operation that the greatest immediate risk is not in the later stages of the operation but at the time of removal of the breast itself. As to the dangers likely to arise in the way of injury to the axillary vessels and nerves, these, in the hands of a skilful operator, are avoidable; and proper care also should prevent any greater likelihood of the wound becoming septic. As judged by the writer's own personal experience, very slight additional risk is run in performing total extirpation of both mamma and glands as compared with removal of the mamma alone; and there is always the sense of satisfaction that the best possible chance has been given to the patient of being rid of the disease. The operation is one unquestionably requiring skill in operating and care in dressing, and unless both these conditions can be vouched for in any series of statistics, any conclusions drawn therefrom are practically valueless. Even the simple statement that the glands of the axilla were removed at the operation is ambiguous. For while one surgeon will mean that he picked out such glands as could be felt another would take it as his

expression for a clean sweep of everything removeable. And to those who act, as Mr. Butlin pleases to stigmatise it, purely upon "theoretical considerations," or, as we prefer to take it, upon pathological facts, the latter expression of the statement is the one alone accepted. The question at present is one upon which no general rules can be laid down, and we venture to think that, notwithstanding Mr. Butlin's statistics, and his deductions therefrom, surgeons will not be led to deviate from the radical measures they are at present inclined to adopt.

Space forbids any further discussion of this subject, but there is one other point which must be briefly alluded to. From the analogy of the lip and some other parts, where only a portion and not the entire part is affected, Mr. Butlin reasons that in suitable cases why should not the tumour (in the mamma) and its immediate surroundings be simply removed, the rest of the healthy glandular tissue be left? There is, perhaps, much to be said for this, and one of not the least forcible arguments, although to a certain extent not a purely surgical one, is, that could women feel that a simple removal of the growth would be practised rather than an excision of the whole gland they would be led the sooner to expose their trouble at its earliest stage rather than to conceal it until graver measures must be taken.

There is much else we would fain notice in this book, but we have already rather overstepped our limits. The work cannot but prove of very great value, as much for the information conveyed as for the incentive it gives towards a more extensive investigation into the results of operations.

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*Vaso-Renal Change versus Bright's Disease.* By J. MILNER FOTHERGILL, M.D. Edin. London: Baillière, Tindall & Cox. 1887.

IN the etiology and pathology of chronic Bright's disease, Dr. Fothergill has hit upon a subject for his pen of surpassing interest and of very great practical importance. Our knowledge of the origin and course of the disease commonly so called has lately, through the labours of many workers, been advancing with rapid strides, and the author has done good service, by the publication of the present volume, in attracting the attention of the profession in general to the progress that has been made. This book will be read by practitioners who have but little leisure for the study of more elaborate and

intricate treatises or special monographs, and to such it will prove valuable if its perusal succeeds in directing their attention to the recent advances in this department of pathology, and causes them to pause in order that they may reconsider their own views, and, if possible, bring them up to date. Regarded from this point of view, Dr. Fothergill's present volume is likely to prove most serviceable, and the end of the author will have been attained if practitioners, after reading it, are led to reconsider their pathological opinions in the way we have indicated.

It is difficult, however, to estimate the real scientific value of this the latest work of one of our most industrious medical book-makers. Personally, we would not be inclined to place it on a very high level, although, no doubt, many will read the book with interest—with what real profit is another matter. Dr. Fothergill is a racy writer, and the raciness of the present volume will no doubt prove to be one of its greatest attractions. Many of its pages read like fiction, but when the book is finished the reader is apt to ask, What is it all about? Flowers of rhetoric are too abundant and oratorical displays too numerous for a serious work on practical medicine. We find it difficult to trust implicitly the scientific accuracy of a book in whose pages we find so many allusions to the "Danish Viking who steered his pirate war-keel to the shores of England," or to "the descendants of the old Cymri rolling back the incursion of the Anglo-Saxon who drove their ancestors into mountain fastnesses." With regard especially to the pathology and pathological anatomy of the volume, in no place does the writing impress us as that of a man who has patiently and perseveringly laboured at the subjects about which he ventures to write. The pathology of the author strikes us as that of an arm-chair pathologist, whose views have been formed in the study rather than in the dead-house and laboratory. Such pathology, however ingenious or plausible, is in no sense the pathology that will satisfy the scientific aspirations of the present day. In his endeavour to unravel the mystery of vaso-renal change, the author has pressed into the service embryology, comparative biology, archæology, and many other "ologies," but when all is done we cannot help thinking that the science of the volume is of a second rate order. The scientific paragraphs remind us of appeals to the gallery, and the reader who is accustomed to the scrupulous exactness of all true scientific research is, in reading them, filled with a vague sense of distrust. Flights of fancy and rounded periods form no part of the serious

scientific writing of our day, and the very facility of the author's pen is his greatest defect in dealing with these parts of his subject.

Turning, however, to the more purely clinical portions of the book, the impressions one receives are much more satisfactory. Here the author stands revealed as a shrewd observer of mankind. His remark that at present the student's attention is too much directed to the disease and too little to the patient is true, and well worthy the serious consideration of all teachers. As regards the physiognomy of vaso-renal disease we have many glowing word pictures, which will be read with pleasure and profit. But, even here, evidence is not wanting that the author, in his observation of disease, has always a "keen eye to the main chance;" and we would recommend all those who wish to impress their patients and to practise profitably—*i. e.*, as regards £ s. d.—to peruse carefully pp. 143-146. Here, in his conversation with the "bright neurotic," the writer—oh, thrice happy man!—represents himself as an intelligent reader of the Almighty's handwriting! Altogether, we think page 145 might very well have been omitted; it savours of pedantic egotism, and no earnest, reverent reader can peruse it without a feeling of repugnance.

The views expressed with regard to the pathology of Bright's disease will, on the whole, find pretty general acceptance. All modern observation and research have been leading up to the opinion that the widely varying symptoms attributed to gout, contracted kidney, arterio-sclerosis, &c., have their origin in a vitiated condition of the blood, due, in the first instance, to imperfect metabolism. This is the keynote of the present volume, and, on the whole, we cannot say that the author has told us anything that was not well enough known before. Legitimate doubt, however, may be entertained of his suggestion that, as regards high arterial tension resulting from a *materies morbi* in the blood, "the first consequence may be really and truly a self-preservative depurative action on the part of the system." This seems to be one of those attractive theories which are apt to fascinate pathologists of the arm-chair order, and which, unless backed up by careful experimental and clinical demonstration, may lead to the propagation of widespread error and malpractice. If there be such a self-preservative action, how is it that the high tension never effects a cure, but continues a more or less prominent sign of the diseased state until the very last stages are reached? The author thinks it "abundantly clear that

the term 'Chronic Bright's Disease,' is no longer adequate, but 'one-sided and contracted.'" In a sense we agree with him, but not for the reason adduced at the beginning of Chapter I, where a remark of Sir James Paget's, that "so long as a disease carries a man's name it shows we know little about it." With all deference to high authority, we think there are several diseases carrying men's names about which we know a great deal. The present volume proves that we know a great deal about "chronic Bright's disease," and no one denies that we know much of the essential nature of Addison's disease. According to the author's own showing, our knowledge of the pathology of chronic Bright's disease is in a transitional state, and before changing the name it would be well to base it on a surer foundation than even Dr. Fothergill has succeeded in laying.

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*The Life and Recollections of Doctor Duguid of Kilwinning.*

Written by himself, and now first printed from the recovered manuscript. Edited by JOHN SERVICE, L.R.C.S. & P.Ed. Edinburgh: Young J. Pentland. 1887.

WE have read this book with interest and with very considerable amusement. It is not the kind of work which usually finds a place among the reviews of a medical journal, but as we have it before us it behoves us to give our reader some idea of its contents. The volume purports to be the recollections of a certain Dr. Duguid of Kilwinning, whose MS. was recovered by the Editor in the garret of an old house which was being pulled down in that town. We rather suspect the finding of the M.S. is a pleasing fiction, and that the author and the editor are the same. We do not know much about Kilwinning, and we therefore opine that the volume will yield its full amount of attraction and interest only to those who are intimately acquainted with this town and its surrounding country. The fact that the recollections are written in the broadest of Scotch will render them a closed book to many readers, but will enhance the pleasure of its perusal to those whose ears are familiar with, and pleased to hear, the broad old dialect of bonnie Scotland. We are scarcely qualified to give an opinion on the literary merits of the book, but in going through its pages it has struck us that the Scotch dialect, though on the whole very good, is not in some places well sustained, and even in the same sentence we come across combinations of modern

English and old Scotch, which would hardly have come from the pen of a man writing the language familiar to him by every day use. The recollections after all are not of a very high order, scarcely even worthy the editor's description of them as the "literary recreations of an educated man in a somewhat illiterate community." We would rather describe them as a series of old Scotch stories, some of them by no means unfamiliar to readers acquainted with Scottish folklore. We notice what looks suspiciously like an anachronism towards the end of the volume, and the chronology of the Burns and Johnson stories struck us as being somewhat confused. The anachronism we speak of is in reference to Laennec. Dr. Duguid's son Willie is represented as returning to Kilwinning an M.D. of Glasgow, with honours, about the years 1812 or 1814, and, as a student before that time, tormenting his father with accounts of the "new lug-horn (stethoscope), and ither playocks of ane Dr. Layneck." Laennec invented the stethoscope in 1815, and its introduction to this country must have been a year or two later than this. We close our notice by again saying that many will obtain an hour or two's genuine amusement from the book, and by giving one or two specimens of its prose and verse:—

"Laird Speckie was a pridefu' gaste of a body. He cam of Stranraer side, but he had been a year or twa in England, some said indeed with the ellwaun' and the pack, but of this I will not positeevly speak. Hoosomever, on the strength of his southern travels, he effected a very scunnersome kin' o' dichter water in his talk. 'What do you suppowse, doctor,' quo' he to me ae day, 'would be the effect of swallowing a brass thumble, thimble,—no, thumble? What do you suppowse now would happen?' 'Weel, laird,' quo' I, 'I have never met wi' a case in a' *my* experience, but the effect o' brass in ony ane's constitution, so far as I have aye been led to believe, is that it helps him up wi' the win'."

"I will here chronicle just one more story about Robin Thughstane. He was very droothy one morning, nothing at all unusual for him; but what was unusual and unexpected, as it was also welcome, was that he met with a stranger in the toon who gave him a dram. In the fulness of his gratitude at the moment, Robin is reported to have said—'Man, I'm vera thankfu' for that, an' I—I'll maybe dae as muckle some day for you! Ye ken I'm the sexton here; dae—dae—dae ye like your heid heich?'"



The following story relates how Dr. Duguid paid back James Gue, the joiner, who was very fond of having a joke at the Doctor's expense:—

"But I found a chance soon after that to hit him just as sairly. James was keen to be a faither, but Jessie White had so far disappointed him. When therefore she did fa' that way, and James cam to me ae morning when she was heavy o' fit, and said, 'Hooray! doctor, hooray! man, oor Jessock's as braid's a gavel!' 'Guidsake, James!' quo I, 'no possible; but Lord, man, steek the door and speak lown, *wha do you suspect?*'"

#### "GARNOCK'S BANKS.

"Garnock's banks hae mony a flow'r  
In beauty blushing braw,  
But, near Saint Winning's ancient tow'r,  
There's ane outshines them a'.

"The lovely language o' the heart  
Lurks in her speaking e'e,  
And, gin ye catch its loving dart,  
Ye'll dream o't till ye dee!

"Sae white her broo, her cheek sae pink,  
Her hair like ony slae,  
Ye'd tak her for a lily-bud  
That's newly blawn in May!

"But haud awa frae the bonnie lass,  
I rede you tent her e'e,  
For, gin you catch its loving dart,  
You'll dream o't till ye dee!"

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*Surgical Pathology and Morbid Anatomy.* By ANTHONY A. BOWLBY, F.R.C.S. With 135 Illustrations. London: J. & A. Churchill. 1887.

WE have much pleasure in recommending this little volume to the favourable consideration of our readers. The work is intended for the use of students of surgery, and we think the author has been successful in producing a compendium of surgical pathology which will prove of much service to beginners. The author's style is clear and concise, and, as is necessary in a work of the kind, matters of controversial or speculative interest have been almost entirely omitted. The strictly surgical pathology of the work is exceedingly good, and proves the accuracy and breadth of the author's knowledge; but in those parts of the book where he has to deal with the

general or medical aspects of his subject he seems to be treading on less familiar ground, and here his descriptions are apt to be defective, and in some places, in our opinion, actually erroneous. Thus, we think, the articles on Fatty Degeneration and Amyloid Degeneration are distinctly insufficient, and the description given of atheroma on page 198 is, according to our experience, never realised. He says:—"Microscopical examination will show that exudation of inflammatory products has occurred, and the cellular tissue beneath the endothelium is seen to be the seat of a considerable collection of leucocytes;" and he then goes on to give reasons, which seem to us exceedingly fanciful, why this particular site should be selected for the inflammatory exudation. Over and over again we have examined sections of atheromatous arteries, and never have we seen the appearances described so fluently by our author. In fact, we doubt much if the process is so clearly an inflammatory one as the writer seems to think. It has given us great pleasure to look through the book, and we are sure that any student who peruses it carefully will acquire a sufficient and very useful knowledge of the general principles of surgical pathology.

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*Smithsonian Reports, 1884, Parts I and II; 1885, Part I.*

THIS epitome of a year's progress in scientific research and experiment, issued from the Government Offices, Washington, presents, in addition to the particular economy of operations, expenditure, and condition of this national institution, in a reliable manner, the condensed extract of the scientific attainments of the year. A few years ago the *Report* was contained in an 8vo volume scarcely three-quarters of an inch in thickness, but now it occupies two volumes of very respectable bulk. That portion of it devoted to the Report of the Secretary records the remarkable progress of the institution, giving details of the various expeditions undertaken by its agents and their results, chiefly referring to geography and natural history, and a full account of the institution, its library, and system of international exchange. The Appendix contains the scientific record for 1884, consisting of able condensations of the progress of astronomy, vulcanology, geography, physics, and allied sciences, together with careful lists of the bibliography for the year of the various subjects referred to.

Part II, which constitutes the second volume, consists of matters affecting the National Museum, contains a general

exhibition of its policy, with details of its staff, condition, and administrative work, with reports of its various sections, also some interesting and exhaustive papers upon collections in it, with lists of papers published by its members, and of the additions to the museum for the year.

Part I for 1885 is based upon the same lines as its predecessor for 1884. We note with pleasure that Congress has voted the sum of £40,000 for a building to be exclusively appropriated for an Army Medical Museum. In the introductory remarks to the Scientific Appendix, the Secretary (Mr. Spencer F. Baird) regrets the omission, for want of space, of the subjects of pathology and medicine, which regret, we hope, will soon be so general as to induce the directors of the institution to take it under their more favourable consideration.

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## MEETINGS OF SOCIETIES.

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### MEDICO-CHIRURGICAL SOCIETY OF GLASGOW.

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SESSION 1887-88.

MEETING I.—7TH OCTOBER, 1887.

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THE first business was the election of Office-bearers, the result of which was given in our last issue.

I.—ON SUBSTITUTION IN DISEASE, ESPECIALLY OF THE NERVOUS SYSTEM, AND THE TREATMENT OF HEREDITY IN THE NEUROSES.

By DR. ALEX. ROBERTSON.

Dr. Robertson remarked that the principle of substitution had a wide range of application within the system. In physiological conditions the association between the skin and the kidneys illustrated this mode of connection. In disease vicarious menstruation was an illustration, and cases were cited from the writer's experience in which the menstrual flux was considered to be from ulcer in the leg, from the stomach, and from the lungs. A case of chronic Bright's disease in the Royal Infirmary, recently under his care, was quoted, in which the stomach was considered to have discharged, to some extent, the function of the kidneys, in the elimination of the constituents of the urine. Gout

sometimes presented phenomena which were explained by this principle. Striking cases of rheumatism and mental disease in the writer's practice, in which substitution was apparent, were quoted. Cases were also submitted in which the healing of bad ulcers was followed by apoplexy, epilepsy, and violent headache. A personal experience was referred to, in which the inflammatory action in the writer's finger had apparently left the bone and periosteum, and been diverted to the surface. It was especially in diseases of the nervous system that this principle was manifested. In very many of the hereditary neuroses, the rule was to change in descent. Thus, epilepsy in the parent, might appear as mania or the insane temperament in the offspring; or the converse might happen, or oinomania in the father or mother might be replaced by epilepsy or insanity in the child. Insanity and asthma were substitutionary in descent, and a recent striking case in the Royal Infirmary was quoted. However, it was pointed out that some diseases of the nervous system were much more stable in descent than others, and particularly that the lower forms of mental defect do not change readily. The question was considered if sanguineous apoplexy has interchangeable relations in heredity.

The writer then passed on to the discussion of substitutionary disease in the same individual. Instances were quoted both from his own practice and that of others, of the interchange between asthma and insanity—asthma disappearing, insanity appearing, insanity passing away, asthma re-appearing, in succession in the same individual. The corresponding relations of neuralgia and mental disease were illustrated. Hysteria and hystero-epilepsy were stated to present many evidences of this protean disposition. Particular reference was made to a patient of Dr. M'Call Anderson's suffering from hysterical anæsthesia, who was shown to the Medico-Chirurgical Society in 1879. She had become insane, and had been repeatedly under Dr. Robertson's care in the City Asylum. Her case was regarded as presenting an excellent illustration of substitution. Morbid action, when anæsthesia was the leading feature, was in the sensorium; when that had passed away and convulsions occurred, that action had been transferred to the motor centres; and when mania was present, the motor and sensory symptoms being in abeyance, there was general disturbance of the hemispherical ganglia. But, besides, morbid action had passed from one hemisphere to the other, when anæsthesia left the

one side of the body, and immediately afterwards appeared in the other.

Observations were made on the pathology of this condition. In some individuals there is general instability of the central nervous system, mind, sensory function, motor and vaso-motor function being all weak and readily disordered; in others the inherited weakness is limited to one part of it, such as to the region of one nerve, *e. g.*, the fifth. But besides a difference in the area of the brain affected, there is a difference in the character or quality of the morbid action. Thus, although most hysterics are emotional and nervous, Dr. Anderson's patient, though a model hysteric, was cool and self-possessed, and sang at public concerts without the slightest perturbation.

In discussing the treatment of heredity, the training of a neurotic child was specially considered. Under this, the kind of diet advisable and what should be avoided; exercise; education, and whether home or public school; methodical routine; religious excitement; tendency to masturbation—counsel to fathers; occupation; marriage; Dr. Maudsley's views; all came under review.

Lastly, reference was made to the stern practice of our forefathers centuries since in dealing with epileptics and and those afflicted with hereditary nervous infirmities and the possible advantage to the present generation.

The discussion on this paper was adjourned till the next meeting of the Medical Section.

## MEETING II.—14TH OCTOBER, 1887.

This meeting consisted in a *conversazione* which was referred to in last issue.

## MEETING III.—21ST OCTOBER, 1887.

PROFESSOR M'CALL ANDERSON, *President, in the Chair.*

### I.—CASES OF KELOID.

DR. ALEX. PATTERSON read the autobiography of a case of keloid and showed another case.

*Dr. Napier* said that Dr. Patterson's case emphasised one fact which is generally admitted with regard to keloid—

namely, the futility of removing the growth in the expectation of obtaining a cure; operation is justifiable only, as in this case, for the relief of urgent symptoms. He supported the view that there is no essential difference between "true" and "false" keloid, and combated the notion that keloid is simply an hypertrophied scar, pointing out the radical differences in structure noticeable in keloid and cicatricial tissue. He also mentioned that recoveries had recently been reported as the result of electrolytic treatment of the nodules.

*Professor McCall Anderson* said that Dr. Patterson's case was very interesting to him, in so far as it illustrated the fact that keloid, after removal, is sure to return again, although he was quite aware that Dr. Patterson had operated upon the patient as a palliative measure. He held that the difference between true and spurious keloid was—that the constitutional tendency to this peculiar form of fibroid hypertrophy was, in some persons, so strong that the disease made its appearance without any apparent exciting cause (true keloid); while, in others, it was not so strong, and did not therefore develop without an exciting cause, such as a burn or the like (false keloid). The true keloid most frequently appeared over the sternum, while the false appeared on any part, *i. e.*, at the seat of the lesion which preceded it. He had at present under his care a patient who had spots and patches of keloid on the sides and back of the neck, and on the front and back of the upper part of the chest, in connection with acne indurations. The last case of true keloid which he had treated—and which, by the way, was seated on the shoulder—diminished in size very much after having the part covered and protected for a good many weeks with belladonna plasters. He believed that multiple scarifications (as recommended by Guyard) or multiple punctures with the galvanic needle (as suggested by Hardaway) were methods of treatment worthy of trial.

## II.—A METHOD OF SECURING AND SUTURING THE BLADDER IN SUPRA-PUBIC CYSTOTOMY.

By A. E. MAYLARD, B.S. (See page 419.)

After some remarks by *Dr. Macewen*,

*Dr. Beatson* remarked that if it was decided to suture the wound in the bladder after supra-pubic lithotomy, there was no doubt about the advisability of passing the stitches through the bladder wall before making the incision into the organ. It was a comparatively easy procedure while the

bladder was distended, but a very difficult one after the organ had been incised and had sunk collapsed into the pelvis. As to the question of suturing the bladder at all, he was not in favour of it. Having recently to perform the supra-pubic operation, he had looked into the point to see what light statistics threw on it, and the opinion he formed from his researches was that it was wiser to leave the incision in the bladder open. He did so in his own case and the result was very satisfactory. One point brought out in the statistics was that in the majority of the fatal cases an attempt at suturing the bladder had been made and had failed. Indeed, it seemed that it was a most difficult matter to suture a bladder in the living person so as entirely to prevent urine coming through. Nothing had been said by Mr. Maylard as to the closure of the abdominal wound. This was of considerable importance in connection with the suturing of the bladder.

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#### MEETING IV.—28TH OCTOBER, 1887.

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##### I.—THE PATHOLOGY OF INFECTIOUS AND INFECTIVE DISEASES AS ILLUSTRATED BY THE FACTS OF INHERITANCE.

BY DR. JOSEPH COATS.

This paper began by stating that the varying susceptibility of different persons to the same infection appeared to some a serious difficulty, especially in the way of accepting the view that diseases of this class are due to micro-organisms. The author sought in the first place to illustrate the fact that inheritance, to a large extent, determines the degree of susceptibility. In illustration of this, he adduced the remarkable immunity of the negro to yellow fever and syphilis and his much less degree of susceptibility to malarial fevers as compared with the white races. As contrasted with this, the author referred to the greater susceptibility of the negro to small-pox, cholera, and phthisis pulmonalis. It was thus apparent that race has a large influence in determining susceptibility to this class of diseases. The points of difference in the various races consist of peculiarities in the structure and function of the tissues, some of them of comparatively little apparent importance, but these peculiarities are matters of inheritance. It was further pointed out that family has a considerable influence on the susceptibility to this class of diseases, and the cases of diphtheria and phthisis pulmonalis were specially

adduced. The peculiarities of families are still more minute than those of races, but these also are matters of inheritance. Having indicated by various illustrations that it is fine structural and functional characteristics which are inherited, the author endeavoured to relate the varying susceptibility to infectious and infective diseases to such characteristics. The living tissues are composed, to a large extent, of active cells, and the diseases concerned depend on micro-organisms, which are also active cells. The relation of these two to each other will vary according to the degree of activity of each, and also according to the individual peculiarities of each. Inheritance plays an important part in determining the kind and degree of activity of the living tissues, and in this way exercises an influence on susceptibility. There are other influences determining variations of susceptibility, but this one of inheritance was important as giving some indication of the kind of influence probably concerned.

*Dr. Alex. Patterson* expressed his high appreciation of the paper and thanked the author.

*Dr. Hugh Thomson* referred to cases of family susceptibility which he had observed. One of them was a striking instance of family proclivity to diphtheria. One child was seized with diphtheria and died, then the mother took it and also died. At the same time two of her children were seized, but they recovered. Then another of the children took it and died, and then a baby which was taken away in order to avoid the infection, strange to say, was seized with the same disease and died three or four months after it came back to the house. But the most striking part of the whole case was this: there were two families in the house by the one father, and it was the children of the second wife who took the disease; there were three or four children of the first wife also living in the house, and not one of them was seized; the father did not take it, nor any of the first family. That is an illustration of susceptibility derivable clearly from one parent. Of course the susceptibility, whatever it may be, is something altogether beyond our sense hitherto. It is certainly very extraordinary how very minute an element in the body, or in a culture fluid may be sufficient either to prevent the growth of a microbe or produce it. I think in the paper which I read some time ago, and which appeared in the *Journal*, it is stated that Monsieur Duclot says that the fifteen-hundred-thousandth of an element in a certain mixture was sufficient to prevent the growth of microbes. I have several times seen small-pox in the negro race, and I never



saw worse cases. They occurred amongst negroes who had not been vaccinated.

*Dr. McCall Anderson* said there is not very much to be said with regard to this paper, except to express our thanks for the way in which it has been drawn up. We are all quite familiar with the influence of inheritance in the propagation of disease, and there is not one at this table who could not, if necessary, give practical illustrations of the cases referred to. The only point in the paper to which he would allude, is the theory which *Dr. Coats* gives to account for this greater liability to certain diseases in certain persons: that is, that the living cells in certain persons, and certain constitutions seem to have a greater power of resisting these microbes, or poisons, than in others. While that may be a true explanation, at the same time, *Dr. Coats* himself would admit that it is a theory, although a very ingenious one; but with the present state of our knowledge we can only register the facts without giving any very satisfactory explanations of how some persons are so liable to contract certain diseases and others are not.

## II.—MICROSCOPIC SPECIMENS OF TRACHOMA OR GRANULAR CONJUNCTIVITIS.

BY DR. C. FRED. POLLOCK.

In showing the specimens, *Dr. Pollock* said that he had placed under the microscopes, examples both of the normal and diseased conjunctiva. The normal conjunctiva contained some adenoid tissue in the sub-epithelial layer. This is scanty in the portion covering the tarsus but more abundant towards the cul-de-sac. Even in the latter situation, however, the round cells with the reticulum form a somewhat diffused tissue without any definite circumscribed arrangement. In trachoma or granular conjunctivitis, on the other hand, this adenoid tissue forms distinct spherical masses in the midst of the inflamed, thickened, and infiltrated conjunctiva, the "trachoma follicles" being composed of round lymphoid cells in a delicate reticulum, and having one of two conditions at their margin, either ceasing abruptly without any bounding membrane, or being surrounded with a kind of capsule formed apparently of the condensed connective tissue, which the little growth had pushed aside.

PATHOLOGICAL AND CLINICAL SOCIETY.

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SESSION 1887-88.

MEETING I.—10TH OCTOBER, 1887.

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*The President, JAMES FINLAYSON, M.D., in the Chair.*

The PRESIDENT delivered an opening address, "On the Function of Medical Societies at the Present Day, and on the Abiding Importance of Pathology to Clinical Workers." The address is published in full in the *Lancet* for 26th November, 1887.

*Professor Gairdner* moved a vote of thanks to Dr. Finlayson for his address.

The following demonstrations were also given:—

(1.) Dr. Thomas Reid showed, in the Council Room, a Wales' camera for micro-photography and a self-registering perimeter.

(2.) Dr. Coats showed Schanze's microtome.

(3.) Dr. Newman showed a phantom larynx and Zeiss' microtome.

(4.) Mr. Maylard showed cultivations and microscopic specimens of bacillus tuberculosis, Koch's cholera bacillus, Friedländer's pneumo-coccus, Rotz bacillus (glanders), bacillus anthracis, and bacillus typhosus.

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MEETING II.—14TH NOVEMBER, 1887.

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*The President, JAMES FINLAYSON, M.D., in the Chair.*

I.—CASE OF FLOATING KIDNEY.

BY PROFESSOR M'CALL ANDERSON.

The patient, æt. 36, was a labourer. Three years before, he felt that he had strained himself while lifting a heavy weight, for he experienced "a snap" in the left side of the abdomen, as if something had given way. He felt quite well, however, and able for his work, until six months afterwards, when one day—when lying down, as was his wont, after dinner—he felt a lump in the left side of the belly. This has persisted ever since, and he has suffered a good deal from pain on

exertion (so that he has only been able to work intermittingly), as well as from uneasiness after food.

On examination of the abdomen, a tumour, about the size of an orange, was discovered a little to the left of and above the umbilicus. It could be moved freely in all directions, especially to the right, and towards the iliac region; but it could not be made to disappear entirely behind the ribs on the left side. It rose and fell in expiration and inspiration. It was smooth, and readily slipped from beneath the fingers. It was the seat of pulsation at times, and on pressure the patient complained of a sickening feeling, such as is experienced by compression of the testicle. The urine was all along quite normal.

The only points which might be urged against the diagnosis are:—(1) The shape of the tumour, it was more rounded and less kidney-shaped than usual; (2) the sex of the patient, the affection being nearly ten times as frequent in females as in males; and (3) the kidney involved, the right being nearly four times as often implicated as the left.

## II.—CASE OF CANCER OF THE KIDNEY.

By PROFESSOR M'CALL ANDERSON AND DR. JOSEPH COATS.

This paper will appear in next month's *Journal*.

## III.—BILHARZIA HAEMATOBIA.

By DR. A. NAPIER, Crosshill.

Dr. Coats, in the absence of Dr. Napier, showed specimens of the ova of the parasite under the microscope, and read the following notes of the case:—

J. F., aged 28, suffered from no illness that he can remember till he went to Egypt on business connected with telegraphic work, in February, 1882. In that country he seems to have enjoyed fair health for about eight months, when he had an attack of piles and internal fistula, ascribed to sleeping three nights in the desert. This was soon followed by his present ailment, the first indication of which was the discovery one morning, of a bright red blood stain, of the size of a half-crown piece, on his shirt. Then daily, for two months, he lost a little blood, bright red in colour, and varying in amount from thirty to sixty drops, with each act of urination, the blood coming at the end of the stream. When this had continued for two months he was suddenly attacked by pain, dull or sub-acute in character, and radiating from a point in

the middle of the lumbar region. With this began the passing of small shreddy clots or sloughs, along with the bright red blood.

At the end of another month, that is, after three months' illness, urination was one day found to be impossible; distension of the bladder became very distressing, and at last, after many fruitless efforts and much hard pressure a gelatinous clot came away. This was about an inch in length, half-an-inch in diameter, cylindrical in shape, and transparent or slightly milky, not unlike jelly. That was the only large gelatinous clot passed, but from time to time since then similar but much smaller clots have come away.

Patient remained in Egypt eighteen months after the time his illness started. During the whole of this time he continued to pass blood and small sloughs; he became very much weaker, and emaciated rapidly, his weight falling from 13 to 9 stones. He was then invalided and sent to Malta, where he stayed five months, and afterwards home, where he remained four months, three weeks of this time being spent in London under Dr. Cobbold's care.

Four months' holiday in this country made a great improvement in his condition; his pain diminished notably, while he became much stronger and his weight increased. He was then sent on duty to Lisbon, where he at once became weaker and much worse in every respect; in particular, the pain in the back was at this time very severe, and seems to have been spasmodic or colicky in character, being relieved by forcibly bending backwards, as over the arm or back of a chair. The liver at this time appears to have been enlarged, and severe pain was felt in the hepatic region. After ten months' duty in Lisbon he had to be invalided once more, and was on holiday again for four months in this country. Then, when he had much improved as the result of the rest and change, he was sent to Land's End, in Cornwall, the climate of which agreed with him remarkably well. Here he grew much stronger and put on flesh, all the time, however, passing more or less bright red blood and small shreddy sloughs as before.

Once while in Egypt, and once again while at Land's End, the whole of the urine passed coagulated immediately in the chamber pot; it was fluid when passed, but dark blood-red in colour. Some of the urine, put into a large bottle, separated into two layers, about 3 inches at the bottom being solid red clot, with a reddish fluid part above.

While staying in Malta Mr. F. became subject to what he

describes as seminal losses, which took place with every act of urination. After some months these emissions became nocturnal, occurring five nights out of six, and were no longer associated with the passing of water. This symptom persisted during all the ten months he was at Lisbon, and ceased only after he was invalided for the second time. There is nothing abnormal at the present time in this respect.

While at Malta, also, he suffered much from irritability of the bladder. Seldom, either by day or night, could he retain urine as long as an hour; more usually he had to urinate every ten minutes, and then, of course, passed only a very little water. This lasted about eighteen months, and disappeared gradually while staying at Land's End.

Within recent months the quantity of blood lost at each urination has greatly lessened; sometimes only a single drop is seen, and it never exceeds about 30 drops at a time.

At present the patient looks well and feels well, and makes no complaint, excepting that he passes the small quantities of blood referred to, with a few small sloughs. He also has a slight sensation of pain or weakness across the small of the back, but it comparatively rarely troubles him unless he sits long at his desk. He has no pain in passing the blood or the sloughs.

Physical examination reveals very little. Heart and lungs normal. There is distinct evidence of enlargement of the liver, hepatic dulness being increased in a downward direction, measuring  $3\frac{1}{2}$  inches in the nipple line. Pressure over the liver causes pain, especially in the situation of the gall-bladder; and the superficial veins in the hepatic region are dilated and unusually well seen. There is sharp pain on pressure over a very limited area in the left lumbar region; the tender part can be covered with the tip of one finger, and is situated at the lower margin of the last rib and about two inches from the spine. No distinct tumour can be made out, but an obscure sense of resistance to pressure.

When first seen by me he was, at his own request, put on salicylate of soda, 40 grain doses at bedtime. This, he thinks, has lessened the loss of blood; at anyrate the bleeding is always greater next day if he misses a dose.

*6th September.*—Eight fluid ounces of clear urine, free from blood, were sent for examination. It was found to be limpid, pale straw-coloured, faintly acid, and free from albumen and sugar; specific gravity, 1012.

A separate sample of urine was also sent, containing a few of the small sloughs. These were found to contain

large numbers of the ova of the bilharzia hæmatobia. The mature parasite itself was not found.

*Remarks.*—The prominent features of this case may be summed up as follows:—Residence in Egypt for about eight months, with no sign of ill-health; then, for two months, the passing of a little bright red blood at the end of the stream of urine and without pain; then the sudden supervention of pain in the back, and the appearance of small sloughs in the urine; the passing of gelatinous clots; great irritability of the bladder, and seminal emissions at first with each urination, subsequently nocturnal only, and afterwards ceasing; emaciation; general debility; improvement, and a return to fair health, with general tonic treatment, and a suitable change of climate; the persistence of the loss of blood, even if slight in amount, and the evidence of the continued presence of the parasite in the urinary apparatus, probably in the kidneys. The patient states that this form of disease is very prevalent in Egypt, and believes that about 50 per cent of the inhabitants of Egypt living along the Sweet Water Canal, suffer from it. This he ascribes to the foul canal water they have to drink.

Treatment by salicylate of soda was suggested by the patient, as a friend of his own who was greatly exhausted by the disease, obtained much benefit from salicylic acid after the comparative failure of the usual climatic treatment. This patient is inclined to think that he was benefited to a slight extent by the salicylate of soda.

#### IV.—CASES OF THORACIC ANEURISM.

By DR. JOHN LINDSAY STEVEN.

Specimens from three cases of thoracic aneurism were shown, which had been under his care while acting in the wards of the Royal Infirmary—viz., (1) Large aneurism of transverse portion, causing severe pressure on the left pneumogastric and recurrent nerves; (2) Large aneurism of the ascending portion, which caused death by rupture into the pericardium; (3) Multiple aneurism of the ascending portion, with very great dilatation of the heart.

An account of the clinical histories and *post-mortem* reports will be found in the next issue of the *Journal*.

GLASGOW OBSTETRICAL AND GYNÆCOLOGICAL  
SOCIETY.

SESSION 1887-88.

MEETING I.—26TH OCTOBER, 1887.

THE following were appointed office-bearers for the year :—

|                                 |       |                                   |
|---------------------------------|-------|-----------------------------------|
| <i>Honorary President,</i>      | -     | PROF. WM. LEISHMAN, M.D.          |
| <i>President,</i>               | - - - | PROF. ABRAHAM WALLACE, M.D.       |
| <i>Vice-Presidents,</i>         | - - - | { J. STUART NAIRNE, F.F.P. & S.G. |
|                                 |       | { MURDOCH CAMERON, M.D.           |
| <i>Treasurer,</i>               | - - - | ROBERT POLLOK, M.D.               |
| <i>Reporting Secretary,</i>     | -     | ROBERT PARK, M.D.                 |
| <i>Secretary,</i>               | - - - | G. A. TURNER, M.D.                |
| <i>Pathological Registrar,-</i> |       | NIGEL STARK, M.B., C.M.           |

*Council.*—J. Marshall, L.R.C.P.E., Coatbridge; John Glaister, M.D.; W. L. Reid, M.D.; P. C. Smith, M.D., Motherwell; David Tindal, M.D.; Skene Keith, F.R.C.S.E., Edinburgh.

## HISTORY OF PESSARIES.

BY DR. E. H. LAWRENCE OLIPHANT.

This interesting and learned paper was illustrated by exhibited specimens of most of the instruments alluded to. He first enunciated the fact that the term was not now used in its original or etymological signification of suppository. Thus, Ambrose Paré had used it. He then traced the manner in which, as the solid vehicle of administration of remedies, it was superseded by the hollow form for injections and fumigations, dwelling on the theories of uterine displacements that dominated gynæcological practice synchronously and the oftentimes absurd manœuvres adopted for their rectification. The works of Avenzoar, Matthew of Ferrara, Spach, De la Motte, Paré, Malgaigne, and Meigs were all referred to and drawn upon in illustration.

He next traced the evolution of the instrument in respect of material and form, and extended applicability. As to the former, he referred to cork and wax (Paré, 1564); nutshell covered with wax (Caspar Bohun Rousset); silver ring with branch stalks (John Bohun, physician to the Duke of Würtemberg); wild vine root covered with wax (invented by a woman) and recommended by Bohun. All the different forms were reviewed and criticised. Intra-uterine pessaries and stems had been condemned at several medical congresses. As to applicability, it was pointed out that their use was confined originally to prolapse, but extends now to versions.

As the paper did not contain controversial matter, there was no discussion, but Dr. Oliphant was accorded a vote of thanks.

## ABSTRACTS FROM CURRENT MEDICAL LITERATURE.

## SURGERY.

BY MR. A. E. MAYLARD.

**The Nature, Origin, and Pathology of Tetanus.** By A. Verneuil. (*Revue de Chirurgie*, 10th October, 1887.)—In this paper the author seeks to elaborate more fully the opinions which he expressed originally at the French Congress of Surgery upon the infectious nature of human tetanus and its animal origin. Professor Verneuil believes the horse to be the common medium for the transmission of the specific virus, and in support of this opinion instances a large number of cases collected from various sources throughout the country. To start upon some sort of basis of investigation, the cases are collected under four heads. In the first class are all those who by profession are in constant contact with horses, such as grooms, stable boys, veterinaries, &c. In the second are placed all those having wounds, irrespective of their profession, who have been brought into passing contact with horses; or those wounded by horses or by objects having some direct relation with them, as harness, carriages, agricultural machines; or whose wounds may have been soiled in some way by the urine or alvine discharges or soil containing these. In the third class are arranged patients, of any profession, who have been in more or less direct connection with other infected animals, whether human or otherwise. In the fourth class, finally, all cases where no connection, direct or indirect, can be traced between the infected and the possible source of infection. The negative cases contained in this last group Professor Verneuil believes will become rarer and rarer as observations are more minutely made. The subject, so far as discussed in the present number of the *Revue*, deals only with the first class of cases, and in illustration no fewer than 111 are narrated. The author observes that this class contains by far the largest number, comprehending more than one half of the total of observations. "This proportion constitutes the most solid basis of my hypothesis."

**On the Operative Treatment of Intestinal Obstruction.** By Dr. Rydygier (*Archiv für Klinische Chirurgie*, vol. xxxvi, 1st part).—This paper consists of the narration of seven cases of intestinal obstruction, upon each of which the author had performed laparotomy. The first case was one of obstruction, due to a loop of intestine being strangulated by a pseudo-ligament. The band was divided between two ligatures, and the loop of intestine drawn out. The patient made a good recovery. The second case was one of ileo-cæcal invagination. The operation was performed 4 days after the onset of the symptoms. The bowel was disinvaginated, but the patient succumbed a few hours after the operation. The third case was one of volvulus of the sigmoid flexure. The operation was performed on the fifth day, and the bowel untwisted. For the first two days progress was favourable, but afterwards head symptoms set in and the patient died apparently of tubercular meningitis five days after the operation. The fourth case was one of strangulation of a loop of small intestine through a sort of membrane which extended across the brim of the pelvis. The parts were not disengaged at the operation, and the patient died a few hours after. The fifth case was obstruction due to a perforation of the cæcal appendix and formation of a fæcal abscess. A piece of intestine had become glued down and constricted. The operation was performed on the fifth day. The abscess cavity was opened and the bowel



liberated. The patient made a good recovery. The sixth case occurred in a new-born child. At the operation, a considerable portion of small intestine was found coiled up and glued together in such a way that there was complete interruption to the lumen of the canal. Twenty-five centimetres were resected. Death occurred a few hours after operation. The seventh case was one of obstruction due to pressure of a carcinomatous tumour upon the sigmoid flexure. The surrounding parts were found too much affected at the operation to attempt resection, so that an artificial anus was made at the lower end of the wound. The patient died the day after the operation.

In commenting upon the subject, the author depreciates delay in the treatment of all cases of obstruction when the symptoms point definitely to a sudden onset. Palliative measures, such as the stomach pump, he regards as useless, and believes that so soon as both the public and profession can be induced to believe in the comparative freedom from danger of the operation of laparotomy, cases will be early treated and better results will be obtained.

**The Osteogenic Factors in the Development and Repair of Bone.** By William Macewen, M.D. (*Annals of Surgery*, October and November, 1887).—"Observation and experimental inquiry prove that the periosteum is not the potent osteogenic factor which many believe it to be. On the other hand, they show that the soft tissues enclosed in the osseous tissue play the chief rôle in the development and reproduction of bone." This brief quotation is sufficient to indicate the drift of Dr. Macewen's remarks. A series of clinical cases are given to support certain propositions. These propositions are:—1. When the periosteum has been mechanically detached from an extensive area of an adult healthy bone, and replaced after the lapse of some hours, union between the bone and the periosteum can take place without sloughing or observable augmentation ensuing. 2. The temporary separation of the periosteum from the bone by inflammatory products is not necessarily attended by death of bone. 3. The periosteum covering a portion of bone may be completely destroyed or permanently removed, yet the denuded bone may not only retain its vitality, but may throw out cells which will cover it and form a new periosteum. 4. A portion of bone which has its continuity severed on all sides, and at the same time has had all its periosteum removed, is capable of living and growing. 5. Not only do detached portions of bone deprived of their periosteum live when re-implanted in their original position, but such portions are capable of living after transplantation. 6. The periosteum does not initiate the reproduction of bone. 7. Bone may be regenerated independently of the medulla, which may itself be reproduced.

In concluding this interesting and instructive paper, Dr. Macewen observes: "The surgeon will not trust the periosteum to regenerate bone unless it has adherent to it sound osseous plaques, the elements of which have the power of proliferation, and from these alone can osseous regeneration proceed. He will not discard injured osseous tissue under the belief that it must necessarily die merely because it is divested of periosteum."

**The Toxic Effects of Iodoform: Cutaneous and Systemic.** By R. W. Taylor, M.D. (*New York Med. Journ.*, 1st October, 1887).—"The extensive use of iodoform in surgical practice renders the subject of this paper of much value. Our limited space permits, however, of only the meagrest outline being given, and we warmly commend the monograph in its entirety to the perusal of all accustomed to make much use of the drug. The author gives a table of twenty-four cases, showing the various skin lesions observed and the constitutional symptoms when present. In all, it appears, a local lesion manifested itself, but only in some did constitutional symptoms present themselves. Among the cutaneous lesions, erythema and eczema were the most constant, while the general symptoms showed much variation. Fever was, however, frequently present, and not a few cases showed head symptoms, such as headache and giddiness, drowsiness and delusions, delirium and nervousness. "The practical lessons taught by the collective knowledge of

the nature and action of iodoform should be well remembered, and may be concisely stated as follows :—

(a.) Its use is indicated—1. On fresh wounds. 2. On diseased surfaces, gangrenous, &c., and on those slow to take on healthy granulation. 3. On the surface of necrosed bone.

(b.) Its use is contra-indicated—1. On freshly cut bone. 2. On granulating surfaces. 3. In cases in which it is known or is found to produce toxic effects.

(c.) 1. It should be dusted on the surface lightly and sparingly. 2. In cavities of any kind it is better to use it in the form of gauze. 3. Such aids to absorption as tightly fitting bandages and impermeable dressings, as also inunction, should not be used. 4. It should be used with great caution in the young and old, in anæmic and neurotic persons, and those suffering from weak heart or Bright's disease : also in very fat and flabby subjects."

**Report of a Case of Pancreatic Cyst treated successfully by Incision and Drainage.** By William T. Bull, M.D. (*New York Medical Journal*, 1st October, 1887).—This case is very fully reported, but we can only draw attention to three prominent features :—the establishment of a diagnosis ; the mode of operating ; and the character of the fluid of the cyst.

1. *The diagnosis.*—"Physical examination excluded aneurism, hydro- or pyo-nephrosis, and established the fact that we had to deal with a retro-peritoneal tumour, as shown by its relation to the stomach and colon. The absence of hooklets, and the hydatid fremitus excluded in all probability an echinococcus cyst of the liver ; and malignant disease of the supra-renal capsules or pancreas was excluded by the evident cystic character of the growth. The history : its first appearance and rapid growth, following an attack of inflammation of the common bile-duct ; its subsidence with the appearance of dark coloured stools ; its reappearance and rapid increase—all pointed to a cyst of the pancreas from inflammatory obstruction of its duct ; while the examination of the fluid taken in connection with the fatty stools and the mellituria made that conclusion a positive one."

2. *Operation.*—A four inch incision was made upwards from just above the umbilicus. The abdominal cavity opened, the omentum was seen covering the cyst. This was torn through. The peritoneum was stitched to the skin with fine catgut in the lower half of the wound, and the skin edges brought together in the upper half. The wound was stuffed with iodoform gauze. On the seventh day after the incision the cyst was opened and drained. The adhesions to the edge of the abdominal wall were left undisturbed.

3. *Character of Fluid.*—Summary—1. Digestion of starch ; 2. Emulsion of fat ; 3. Spectrum of a blood pigment (methæmoglobin) ; 4. Large quantity of sugar—2.70 per cent ; 5. Large quantity of serum albumen—2.21 per cent ; 6. Large quantity of free cells resembling leucocytes.

**The Use of Hot Water in Surgery.** F. R. Varick, M.D. (*New York Med. Jour.*, 15th October, 1887).—In a paper, previously presented by the author, on the "Protective Treatment of Open Wounds," before the New York County Medical Association, in February, 1885, the principles involved in the method of treatment of wounds by hot water were more fully discussed, and the results of experiments demonstrating the protective power of a film of coagulated albumen were shown. The present paper, read before the New York State Medical Association, purports to be a review of two years' further experience obtained by this method of treating wounds. The application of hot water checks the outward flow of fluid from the tissues, coagulates the albuminous elements in the fluid and forms an impenetrable shield over the surface of the wound. Hot water, applied to the abraided surface, acts as a powerful cardiac stimulant and controls shock. In making the application, water slightly below the boiling point should be used. The application should be continued until *all oozing* is stopped, the parts are thoroughly glazed, and the red hue of the tissues is decidedly deadened.

## EPIDEMIOLOGY.

By JAMES W. ALLAN, M.B.

**Diphtheria Conveyed by Milk.**—Mr. Power, in a *Report to the Local Government Board on an Outbreak of Diphtheria at York Town and Camberley*, argues that the *quantity* of the milk used and the *frequency* of use, as well as the treatment of the milk after reception into the house, had an important influence in determining infection. The "better" classes, who consumed most milk, doubtless took it most frequently. They also "stored" the milk in the house. Those "better" classes suffered most severely.—(*Practitioner*, July, 1887.)

**Cases of Milk Poisoning Probably due to a Ptomaine.**—In the *Practitioner* for July, 1887, Surgeon R. H. Firth reports cases of milk poisoning which occurred in the European infantry section of the Station Hospital at Meean Meer, the symptoms being "nausea, vomiting, dryness, and intense constriction at the fauces, vertigo, colic, purging, and in some a tendency towards collapse, in others numbness of the extremities and disposition to stupor." Nine of the ten men in the military prison were similarly affected. All the cases recovered.

Setting to work on the milk, Dr. Firth succeeded in obtaining, after a series of processes, "a moist semi-crystallised residue" which possessed "a mawkish, sickly odour, with a strong pungent taste when put on the tongue. Trying some cautiously myself, I was soon conscious of marked nausea and dryness of the throat, followed by headache."

Dr. Firth tried several experiments, which need not be detailed. Suffice it to say that, by suspending a bit of stale butter in some sound milk, he so affected the latter as to be enabled to obtain from it a substance "identical with the toxic residue isolated from the original milk. The conclusions which I have, therefore, drawn are—(a) the toxic product isolated from the original and from the other samples of milk are identical; (b) that this product is probably a ptomaine; (c) that this body is probably dependent upon the action of butyric or some other fatty acid upon decomposing nitrogenous matter; (d) that a continuous temperature of 80° F. and upwards is necessary for its formation."

**"Vaccination in the Harem.**—The women in the Sultan's seraglio at Constantinople have just been vaccinated to the number of 150. The operation took place in a large hall, under the superintendence of four gigantic eunuchs. The Italian surgeon to whom the task was confided was stationed in front of a huge screen, and the women were concealed behind it. A hole had been made in the centre of the screen just large enough to allow an arm to pass through; and in this manner the arms, of various colours and sizes, were presented to the operator in rapid succession. It was utterly impossible for the surgeon to get a glimpse of his patients; but, in order to guard against the chance of his being able to see through the screen, two eunuchs who stood by the operator threw a shawl over his face the instant an operation was concluded, and did not remove it till the next arm had been placed in position."—(*The Indian Medical Gazette*, June, 1887.)

**Note on the Discovery of a Micro-Organism in Malta Fever.** By Surgeon David Bruce, M.B.Edin., Station Hospital, Valetta, Malta.—The writer states that Malta fever, under different names, is common in the Mediterranean. The disease runs a long course and is attended with high temperature. It may be continued, intermittent, or remittent in character, or it may pass from one type to another. Enlargement of the spleen is one of the features of the disease. There is no bowel lesion. "The mortality is, as a rule, exceedingly small."

In one of his experiments, Dr. Bruce "inoculated eight tubes containing sterilised agar-agar nutrient jelly" (with "the usual precautions"). "A platinum needle, heated to redness before each inoculation, was used to convey a small portion of the spleen pulp to the solid jelly." Six of the eight tubes were subsequently put into an incubator with a temperature of 37° C. The other two tubes were exposed to the atmospheric temperature. "In all these eight tubes one and the same growth appeared. It appeared simultaneously in all the tubes placed in the incubator after a period of 68 hours. The two tubes kept at the ordinary temperature of the air only showed signs of growth at the end of 168 hours. The growth appeared at first as minute pearly-white spots scattered round the point of puncture. Small round colonies could be seen along the needle track. After several days these spots grew somewhat larger, and joined to form a rosette-shaped colony on the surface. There is no tendency to liquefaction of the nutrient jelly, and after the lapse of 21 days the colonies have not spread over the whole surface, but remain restricted to an area of, on an average, '25 of an inch . . . . When a minute portion taken from one of these colonies is placed in a drop of sterilised water and examined under a high power, innumerable small micrococci are seen. They are very active and dance about, as a rule, singly, sometimes in pairs, rarely in short chains. All the tubes were examined, and found to contain the same organism."

In another experiment and another case, Dr. Bruce examined the spleen pulp under 500 diameters and found that "the field of the microscope was literally crowded with myriads of micrococci dancing about in the most active manner."

Dr. Bruce considers that he has proved—"(*a.*) That there exists in the spleen of cases of Malta fever a definite micro-organism; and (*b.*) that this micro-organism can be cultivated outside the human body."

**"The Diagnosis of Tropical Enteric Fever."**—Under this title, Dr. G. Harrison Youngue contributes an interesting paper to the *Indian Medical Gazette* for June, 1887. Among the peculiarities of enteric fever in India may be mentioned the following:—

In most cases "sudden onset;" "precedent epistaxis" is "rare in the tropics;" we have dusky, flushed face, suffused eyes, and heavy look like what is seen in typhus. As to temperature, "it is not uncommon in the hot weather to see cases in which the temperature on the first evening of observation reaches 104° or more. . . . The maximum temperature may be reached at noon, instead of in the evening." "Hyperpyrexia is much more frequent in the tropics." The "rose-coloured rash," when present, may be obscured by the "prickly heat," &c. The somewhat slow rate of the pulse is remarkable. "It rarely exceeds 100 per minute, and in the gravest cases may be little above normal"—(perhaps due to depressing effect of malarial poison on the heart). The classical "pea-soup diarrhoea" is "extremely rare in the tropics." Among the varieties of stool found in "tropical enteric fever" are those of "a bright yellowish orange colour" and those "varying from a light unhealthy grass green to a dark slate colour" (*Mem. Ochrey, and green stools are not unknown in enteric fever, as it occurs in Scotland*). The enlargement of the spleen does not help in diagnosis so much as it would in a non-malarial country. The writer believes intestinal hæmorrhage to be "much more frequent in tropical climates." In the more serious form, which Dr. Youngue believes to occur in scorbutic subjects, there is "found a peculiar punctiform congestion of the mucous membrane of the large intestine, especially of that of the descending colon and rectum. The hæmorrhage appeared to be due to general oozing from the mucous membrane of these parts."

Dr. Youngue strongly recommends the addition of "digestive ferments" to the milk given to enteric patients. Pepsine (or pancreatine) should be put into the milk and the latter should be warmed to 100° and kept at that temperature for half an hour.

**"On Sea-borne Cholera: British Measures of Prevention v. European Measures of Restriction."**—Such is the title of an excellent paper read by Dr. Thorne Thorne at the Dublin meeting of the British Medical Association this year. It is written by one who is a master of the subject. Dr. Thorne Thorne thus defines and contrasts the "Preventive" and "Restrictive" systems:—"The one method, which is essentially restrictive in character, has always been advocated by several of the Governments of Southern Europe, France invariably taking a prominent lead. The other, which is essentially preventive in character, has always been aimed at, and more or less insisted on, by this country. The one is the quarantine system; the other is our system of medical inspection and isolation. The one says to Cholera, 'Hitherto shalt thou come, but no further;' the other, based on experience as to what is practicable in controlling the march of such a disease as cholera amongst civilised communities, has, above all things, aimed at the removal from the midst of the people of those conditions which are essential to the spread of the infection in question." After remarking that in all likelihood Europe's greatest risk is from sea-borne cholera, Dr. Thorne Thorne asks us to imagine what would be the result of a thorough and consistent application of the quarantine system to the enormous stream of vessels from Eastern ports? He points to the recent results in the case of France, the great "quarantining country." He goes on to describe the miserable farce of "quarantine" in the Red Sea, than which nothing could be further removed from a "sanitary" measure. The British plan of "inspection," "isolation," and "local sanitary administration," is practicable and reasonable, and has shown itself to be worthy of confidence and support.—*The Practitioner*, October, 1887.

**"Notes on an Epidemic of Pneumonitis."** (*Indian Medical Gazette*, April, 1887).—Dr. Alexander Faulkner records an outbreak of pneumonia which occurred in the 19th Bombay Infantry after a march from Rajpootana to South Afghanistan. The march occupied a fortnight.

The symptoms were pyrexia, cough, pain in chest, rusty sputum, &c. Physical examination revealed dull percussion and crepitation. *Post-mortem* examination showed red hepatisation, &c.

Dr. Faulkner thinks he is justified in using the term "epidemic," seeing that from 8th December, 1886, till the termination of February, 1887, there were "42 cases of acute pneumonia, besides "105 cases of simple and bronchial catarrh." He does not favour the "contagious" theory, but thinks the disease is due to "climatic influences." The Hindus suffered most; they furnished 40 out of the 42 cases of pneumonia. They cook their food out of doors (after taking a bath) without protecting their chests with clothing, and they avoid meat and very seldom take stimulant. These restrictions are due to caste. The weather in South Afghanistan is cold in December and January.

**"The Recent Epidemic of Diarrhoea."**—Under this title Dr. B. W. Richardson, in the *Asclepiad* for the second quarter of the present year, calls attention to a recent outbreak of intestinal disorder "in London and the provinces." Dr. Richardson has been unable to discover "any reasonable cause for the attack" in any of the cases which have come under his observation. The disease has been characterised by abdominal uneasiness and distention, this being "suddenly" followed by "copious and brisk discharge of colourless fluid from the bowels, with much flatus, without either griping or spasm, but with immediate sense of relief from the oppression," &c. He concludes by saying—"I am inclined to think with Mr. Blyth, that the affection is due to some obscure atmospheric peculiarity. I have not found a particle of evidence that would connect it with impure water, with diseased milk, with any article of diet, or with drainage. The first physical derangement in those who have suffered from the disorder has been hepatic congestion, with deficient secretion of bile." . . . "The epidemic has prevailed mostly amongst the adult members of the population. Children have been remarkably exempt from it."

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